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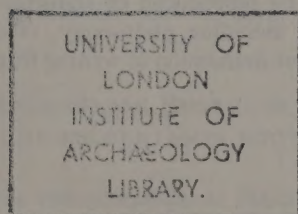
UNIVERSITY OF LONDON

BULLETIN

of the

INSTITUTE OF ARCHAEOLOGY

Number nineteen: 1982



Published at the Institute
31-34 GORDON SQUARE, LONDON WC1H 0PY
1983

© 1982 The Institute of Archaeology

ISSN 0076-0722

Published by the Institute of Archaeology
31-34 Gordon Square, London WC1H 0PY

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Printed by Adlard and Son Ltd, Bartholomew Press, Dorking, Surrey

Contents

	PAGE
If Childe Were Alive Today <i>by</i> BRUCE TRIGGER	1
Gift Exchange and the Hallstatt courts <i>by</i> KARL KROMER	21
Prehistoric Saltmining at Hallstatt <i>by</i> F. E. BARTH	31
The Exploration of the Long-distance Movement of Bronze in Bronze and Early Iron Age Europe <i>by</i> J. P. NORTHOVER	45
The Relevance of Chemical Provenance Studies to Celtic Ironwork in Britain <i>by</i> C. J. SALTER	73
Hallstatt D Daggers: Britain and Europe <i>by</i> E. M. JOPE	83
Rescue Archaeology in Sussex 1981: An Eighth Progress Report on the Sussex Archaeological Field Unit <i>by</i> OWEN BEDWIN, DAVID RUDLING <i>and</i> ANDREW WOODCOCK	91
'The Secondary Products Revolution' and the Limitations of the Neolithic <i>by</i> J. C. CHAPMAN	107
Computer Recording of Archaeological Excavations <i>by</i> IAN GRAHAM, J. C. MOFFETT, KEVIN FLUDE <i>and</i> BENJAMIN BOOTH	123
Experimental Studies in the Determination of Flaking Mode <i>by</i> KATSUHIKO OHNUMA <i>and</i> CHRISTOPHER BERGMAN	161
Whether Pits be Vats: Some Further Interpretations of Subterranean Features <i>by</i> CHRISTOPHER EVANS	171
Book Reviews	177
Reports	241
The Contributors	243
Notes to Contributors	245

If Childe Were Alive Today*

by BRUCE TRIGGER†

In the 25 years since Gordon Childe ended both his life and his career, the world in general and archaeology as a discipline have experienced more changes than during any previous period of equivalent length. The number of professional archaeologists has increased dramatically and for the first time many of them have come to be employed outside universities and museums. Growing awareness of the destructive side effects of economic development has made governments and the general public more sensitive to the need to protect their cultural heritage and, as a result, many countries have increased their allocations for archaeological research. For the first time, this research is also being carried out by archaeologists who are employed not only in Europe and North America but in most regions of the world. Alongside these institutional developments, new analytical techniques have greatly enhanced control of chronology and the ability of archaeologists to extract behavioural information from their data. Not surprisingly, such rapid changes have led not only to an extraordinary enrichment of our knowledge of the past but also to numerous controversies concerning the nature and goals of archaeology.

Controversy also surrounds the continuing value of Childe's work. Some archaeologists see in his writings concepts that remain worthy of further elaboration (Gilman, 1981). Others cite him merely as the advocate of outmoded analytical concepts, such as diffusionism (Daniel, 1981: 162–3) or a normative view of culture (Chapman *et al.*, 1981: 3–4). Given the vast number of works that he produced and the changes in his views over a 40-year period, it is not difficult, by citing passages out of context, to cast him as a straw man in many mutually contradictory roles. In this paper, I will attempt to evaluate the lasting importance of his work by considering what some of his reactions might have been had he been able to witness the changes that have occurred in archaeology since 1957. Doing this requires a comprehensive view of Childe's thought and its development.

I freely admit that the present exercise involves a number of unresolvable problems. I never knew Childe, although what archaeologists who did have said about him suggests that this is not the disadvantage it might be with some other scholars. I am also removed from him by a gap in time and cultural background and have experienced archaeology

* The fourth Gordon Childe Memorial Lecture given at the Institute on 28 April 1982.

† Air travel to deliver this lecture was covered by a grant from the Faculty of Graduate Studies and Research, McGill University.

at a different stage in its development. These differences are probably great enough to create misunderstandings, yet insufficient to promote dispassionate objectivity. As a result, I may be tempted to attribute to Childe more of my own thoughts and prejudices than are warranted.

Yet I would suggest that this is not a wholly speculative enterprise. In recent years various scholars have begun to systematise and evaluate what can be known about Childe's thought from his life and writings (Trigger, 1980a; McNairn, 1980; Green, 1981). At the end of his career, he also prepared two brief but remarkable documents. In the one entitled 'Retrospect' (1958a), which he posted to Professor Grahame Clark from Australia a few weeks before his death, he assessed his accomplishments in the field of archaeology with a remarkable degree of candour and objectivity. Careful study has revealed only a few minor points about which Childe's memory deceived him and, in general, has confirmed his judgements of his own work. In a companion piece, written shortly before his death, Childe (1958b) summarised his conclusions concerning 'the main tasks confronting archaeology in Britain'. These conclusions have world-wide applicability. Although the editor titled this piece 'Valediction', it might better have been called 'Prospect' to indicate its complementary relationship to 'Retrospect'. Both works, in fact, add up to Childe's Valediction. Whatever it is called, this latter work provides a firm basis for understanding how Childe, as a spectator, might have judged current trends in archaeology.

European prehistory

Many British archaeologists, who view Childe primarily as a European prehistorian, would no doubt conclude that for him the most shattering development of the last 25 years would have been the rejection, as a result of the new calibrated radiocarbon chronology, of Oscar Montelius's diffusionist hypotheses, which had structured much of his thinking about European prehistory (Renfrew, 1973a). Yet I doubt that the results would be all that devastating. Few archaeologists were more aware than Childe of the need for a 'reliable absolute chronology' (1958b: 1) or that the archaeological chronologies available prior to the 1960s did not permit any certainty in dating with respect to prehistoric Europe and the Near East. Throughout his career, he acknowledged a longer (though, as it turns out, not long enough) European chronology as an alternative to his preferred short one and freely discussed its implications with his students (Green, 1981: 114). Towards the end of his life, he recognised that the recently invented technique of radiocarbon dating might soon provide archaeology with a reliable, world-wide chronology 'independent of . . . any historical assumption' (1958b: 1-2). He wisely maintained that before archaeologists could rely on radiocarbon dates they would have to understand the method in sufficient detail 'to appreciate the limitations of the information [it could] provide' (1958b: 2). There is no evidence that he imagined that doing this would result in radiocarbon dates prior to 1500 BC being pushed even further back in time. It nevertheless seems clear that he feared that radiocarbon dating might

provide definitive support for a long chronology and that he was preparing himself to cope with this unwelcome possibility (Childe, 1950).

Childe was a fearlessly objective scholar who, we can be sure, would never have clung stubbornly to any interpretation in the face of clear evidence to the contrary. He would have defended his short chronology for as long as it seemed reasonable to do so. Yet he would not have done this blindly and in the long run he would have acknowledged the new chronology and many of Colin Renfrew's (1979) attendant reinterpretations as major progress in the understanding of European prehistory. While he realised that his theoretical work was closely related to his studies of prehistory, in 'Retrospect' he insisted that his most useful and lasting contributions to archaeology were not his interpretations of European prehistory, which he warned might be 'ill-founded', but rather his 'interpretative concepts and methods of explanation' (Childe, 1958a: 69).

Childe would have identified the most painful implications of the revolution in the understanding of European prehistory at the ideological and, more profoundly, at the moral level. For him, the concept of diffusion had strong ethical implications. It expressed the mutual interdependence of all mankind and was a powerful weapon for opposing doctrines which asserted the superiority of one racial or ethnic group over another (Childe, 1933). He believed that demonstrating that Europe owed its early technological development to the diffusion of knowledge from the Near East served to counteract the myths of European, and specifically Teutonic, superiority that played such an important role in European politics during most of his professional career and which had been introduced in an overtly political fashion into archaeology by the nationalistic interpretations of prehistory of the German archaeologist Gustaf Kossinna. Childe's insistence on the uncreativity of prehistoric Europe and the debt its peoples owed to the Near East markedly increased during the 1930s in response to the threat of National Socialism (Childe, 1958a: 72); while a more balanced view reasserted itself as racism and ethnocentrism receded following World War II (Childe, 1958c).

Childe certainly would have been fascinated by the current tendency to advance claims of multiple inventions of numerous items of material culture, even for neighbouring regions, and to discern parallel developmental sequences in different parts of the world (Renfrew, 1979; Adams, 1966). He would have relished the irony that, on this particular issue, modern Western interpretations are in broad agreement with orthodox Marxist theories of the 1930s, which he, with his strong late Victorian heritage of doubts about human creativity, had not been prepared to accept. He might have interpreted this as chastening evidence that he should have been more critical of non-Marxist views as they influenced his interpretations of archaeological data. More certainly he would have identified the current position as being one direction in which his post-war studies of cultural evolution and his slowly growing appreciation of the comparative value of archaeological data from South-east Asia and the New World were taking him (Childe, 1951: 26-7; Green, 1981: 149).

Yet I am convinced that Childe never would have been prepared to abandon the concept of diffusion as a justifiable cover term for a whole series of human interactions

that he regarded as being of fundamental importance for understanding human history. He certainly would not have espoused the currently fashionable view that diffusion is a 'non-principle' and the contention that it has not played a significant role in shaping cultural development. On the contrary, he would have approved of David Clarke's (1979: 94) observation that 'just as "invasion" explanations were conditioned by the metaphysics of short chronologies... so "autonomous" explanations become meaningless among networked communities... — invasion "catastrophism" can be joined by the currently fashionable... "spontaneous generation"'. Childe would also have continued to argue that the breakdown of cultural isolation and the sharing of knowledge derived from many different cultures were major factors promoting cultural progress, originally on a regional and more recently on a global scale. Like David Clarke and the American anthropologist Harold Driver (1974), he would have regarded the role played by diffusion, no less than that played by local development, as worthy of continuing empirical study.

Marxist archaeology

Soviet archaeology played an important catalytic role in the development of Childe's thought and, while it may not currently be popular in the West to say so, I believe that he would be pleased by the progress that has been made in this branch of archaeology since the Stalin era. Childe was generally muted in his criticisms of the Soviet Union, because he did not wish to encourage and assist those who were inveterately, and in his opinion unjustifiably, hostile to it (Green, 1981: 121). Yet it is clear from his published works and even more so from his personal correspondence that throughout his career he had specific reservations about both the Soviet government and its archaeological programme. From his first visit to the Soviet Union in 1935, he admired the emphasis that archaeologists there were placing on the reconstruction of social organisation and explaining change 'without appeal to undocumented external factors' (Childe, 1958a: 71–2). Yet he was aware of government constraints on freedom of academic enquiry, even within an explicitly Marxist framework, and later privately rejoiced when he believed that there was evidence that these constraints were being relaxed or when he encountered individual Soviet scholars who had the courage to pursue a line of research that was contrary to them. In a letter written to Robert Braidwood, following his visit to the Soviet Union in 1945, he reported with delight that he had encountered a student working on skeletal material from a Fatyanovo cemetery who told him she believed that it indicated a migration. He added 'Ten years ago this suggestion might have been penalised as fascist so although not illegal the evidence must be very convincing' (letter of Childe to Braidwood 1.8.45).

From the beginning, Childe also registered a series of very specific disagreements with the Soviet archaeology of the 1930s. He never employed, even in *Scotland before the Scots* (1946a), the detailed scheme of unilinear social evolution that Soviet historians and archaeologists had derived from the work of Friedrich Engels. This scheme conflicted

with his own multilinear view of history and, in any case, seemed to him from a scientific viewpoint to be something that should be proved rather than assumed in advance to be a correct basis for interpreting archaeological data (Childe, 1951: 29). Childe did, however, find the way in which Russian archaeologists 'applied the general theory' to specific problems 'helpful in many points' (letter of Childe to Braidwood 1.8.45). He also doubted that ethnographic analogies could be used to interpret archaeological data in as simplistic a fashion as Soviet archaeologists thought possible. This aspect of their work likewise seemed redolent of the discredited unilinear evolutionism of the 19th century. While he heartily approved of Soviet archaeologists' efforts to explain changes internally, he rejected Nicholai Marr's repudiation of diffusion and migration as valid historical processes. He believed too that in the long run the rejection of typological studies, on the grounds that they were a form of middle class obscurantism that impeded the societal interpretation of archaeological data, could only impair a necessary understanding of chronological and spatial variation in the archaeological record (Daniel, 1958: 66). Childe believed that while Marxism might provide important guidance for understanding the historical significance of archaeological findings, it did not provide a methodology for recovering and processing archaeological data. That required a discipline of archaeology based on principles that could be shared by all archaeologists regardless of their political or philosophical orientations. Childe was somewhat relieved when archaeology re-emerged as a distinct discipline in the Soviet Union in 1935, after a period when it had been classified only as a type of data to be used by historians (Childe, 1940). Yet this development did not answer the other objections that he had raised against it.

Largely as a result of bitter personal experiences in Australia during and after World War I, Childe was a political realist who had few, if any, utopian illusions. Yet he was prepared to take comfort from such real progress as does occur in social and political affairs. I believe that he would be pleased by the genuine improvements that have taken place in the quality of Soviet life as a result of the gradual relaxation of many rigid controls since the Stalin era, especially as these are reflected in the social sciences by growing diversification of ideas and tolerance of debate within a Marxist framework. This is evident in the abandonment of the dogmatic form of the theory of stages (Klejn, 1977: 12–14) and the resumption of debates concerning the usefulness of the concept of the Asiatic Mode of Production after a long period when all discussion of that concept had been banned for political reasons (Bailey and Llobera, 1981: 47–106). Childe also specifically would have approved of the growing attention that various groups of Soviet archaeologists are paying to artefact classification and chronology building, ecology, diffusion and migration as factors explaining historical change, and the need for a distinctive body of method and theory to process archaeological data so that the results can be used more effectively to promote a Marxist analysis of human history (Bulkin *et al.*, 1982). While there is no monolithic agreement among Soviet archaeologists concerning the value of these diverse new trends, Childe would have seen each of them as a positive development that is helping to remedy specific defects that he had noted in an earlier phase of Soviet archaeology. He would also have welcomed the increasing di-

versity of approaches that characterise modern Soviet archaeology as being a healthy development in and of itself. No doubt he would have found it gently ironic that Soviet archaeologists are slowly perceiving the value of concepts similar to those found in traditional Western archaeology at the same time that Western archaeologists have turned away from these concepts towards the study of internal social change. This no doubt explains why Leo Klejn (1977), in his 'Panorama of Theoretical Archaeology', wrote so approvingly of traditional Western archaeologists, such as Grahame Clark and Irving Rouse, while severely criticising many aspects of the New Archaeology.

Childe also would have been pleased by the growing interest in Soviet archaeology in Western Europe and the limited but significant influence that it has exerted over research being done there (Audouze and Leroi-Gourhan, 1981: 172; Spriggs, 1974). He would have been even more gratified, however, by the spread of a Marxist approach to archaeology in Latin America. This has occurred mainly as a result of the influence, not of Soviet archaeology, but of Childe's own writings, as they were popularised by scholars such as Pedro Armillas and José Lorenzo in Mexico and Emilio Choy in Peru. While the original work closely resembled Childe's evolutionary writings, recent studies have become more diversified. Some are rather uninspired efforts to apply Marxist schemes of analysis to archaeological data; others, such as Luis Lumbreras's (1974) *Arqueología como Ciencia Social* or L. F. Bate's (1978) study of the value of the concept of culture for Marxist archaeology are works of creative scholarship. While Childe would have been sympathetic to the ideas of the French Marxist anthropologist Claude Meillassoux, I believe that he would have been less impressed by the influence on Marxist archaeology, both in Latin America and England, of Maurice Godelier (Godelier, 1980). He would quickly have identified the latter's work as Durkheimian social anthropology using Marxist terminology rather than as genuine Marxism.

Ecology

Even if Childe made few original contributions to the analysis of archaeological data from an ecological perspective (Trigger, 1980a: 84–5), he appreciated the importance of such work and would have approved of the spectacular advances in ecological interpretation over the past quarter century. He would applaud the routine recovery and analysis of floral and faunal data and the systematic efforts that are being made to translate such data into information about subsistence. He would also approve of efforts to interpret settlement patterns in terms of subsistence practices and to determine the functional significance of artefacts. No doubt, however, he would point out the weaknesses of such approaches, especially at higher levels of analysis. These would include demographic explanations of social change in the absence of adequate direct measurements of population size or sufficient chronological controls to correlate changing population densities objectively with other aspects of the archaeological record (Hassan, 1978). His reaction would be sharpened by memories of his own archaeologically unsubstantiated and now widely rejected claims about such relationships

in prehistoric times. He would also reject the use of general ecological theory as an explanation of human behaviour because it is unjustifiably deterministic and reductionist.

Societal archaeology

Of considerably more direct interest to Childe would be the many varied and far-reaching efforts to provide sociological interpretations of archaeological data. This was an aspect of archaeological research that Childe regarded as being vitally important; yet which he was very pessimistic about in his later years (Childe, 1951: 34, 54–71; 1958b: 3–4; 1958c: 12–14). Perhaps the most striking single development in this field has been the emergence of settlement studies in North American archaeology. Gordon Willey (1953) had pioneered this approach in his analysis of changing settlement patterns in the Virú Valley of Peru, published four years before Childe died, but it has since been developed and refined by the work of Robert Adams in Iraq, K. C. Chang on China, William Sanders, René Millon, and others in Mesoamerica, as well as by archaeologists working elsewhere. Already in the 1930s, S. P. Tolstov had begun to study the distribution of different types of settlements in Soviet Central Asia in order to discern how whole societies had functioned at different periods (Kohl, 1981: viii). Childe had himself helped to pioneer this approach still earlier with his interpretations of prehistoric life at Skara Brae (1931) and the social significance of neolithic settlement patterns on the island of Rousay (1942b).

Childe would also be interested in the use that has been made during the last decade of patterns of funerary activities and medical examinations of skeletal material to infer prehistoric social groupings and especially social hierarchies (Chapman *et al.*, 1981). This was another field to which, inspired by the work of the Soviet archaeologists A. P. Kruglov and G. V. Podgayetskiy, he had made some pioneering contributions in the 1940s (Childe, 1944, 1945a). Less familiar, but no less interesting to him would be the debates concerning the use of correlations and distributions of stylistic attributes to infer residence patterns and other aspects of the social organisation of prehistoric communities (Plog, 1980) or the use of formal spatial analysis, sometimes in combination with trace-element studies, to interpret trading patterns and interregional communication networks (Flannery, 1976).

Childe would also approve of the growing tendency to view networks of social relations as the primary objectives of archaeological study, with material culture being seen as acquiring its functional significance only as it relates to such patterns. He had pioneered this approach, again initially under the influence of Soviet archaeology, in *Scotland before the Scots* (1946a), *Social Evolution* (1951), and *The Prehistory of European Society* (1958c). It has since been carried further, though without ideological connotations, by settlement archaeologists in North America and by Colin Renfrew (1973b) in England. In recent years, it has also become an increasingly important approach for processual archaeologists in the United States, among whom the concept of

social system seems quietly to be gaining ground at the expense of cultural systems (Redman *et al.*, 1978).

Experimental archaeology and ethnology

Childe would almost certainly approve of the growing importance of experimental archaeology, especially efforts to determine by means of controlled replication how stone, bone, and other tools were made and used. This is a research tradition that goes back to Sven Nilsson's (1868) concern with the value of use marks as a means of verifying the function of prehistoric stone tools. It is also a type of study to which Childe and W. Thorneycroft made a notable contribution in the two experiments that they carried out in 1937 concerning the origin of vitrified forts (Childe, 1938) and for which the Soviet archaeologist S. A. Semenov (1964) gained world-wide recognition as a result of his development of use-wear analysis.

It is less certain what Childe's reactions to ethnoarchaeology would be. Almost certainly, he would agree in principle that we must know more about the correlation between material culture, on the one hand, and social organisation and cognitive factors, on the other, if we are to infer the latter more effectively from the archaeological record. Yet, except where a direct historical or folkloric approach can be applied, the explanatory power of ethnoarchaeology is usually closely linked to the acceptability of a neo-evolutionary position (Gould, 1980). Only if we can assume that societies having what we consider to be similar types of economies will also share similar cultural patterns, with the resemblances being more striking if they also occupy similar environments, will ethnoarchaeological studies of modern non-industrial societies necessarily result in generalisations that significantly increase our ability to interpret prehistoric archaeological data. Childe frequently asserted in his writings that archaeology had the same relationship to ethnology that palaeontology had to biology (1946b); a statement that is often wrongly construed to mean the same as the Americanist slogan that archaeology is the past tense of anthropology. He appears to have meant that archaeological and historical data are the only appropriate bases for studying cultural evolution. In his writings, he argued with varying degrees of intensity against the idea that modern, so-called 'primitive' societies are reliable analogues of prehistoric ones (Childe, 1936: 52-3).

During the 1960s, when neoevolutionary views predominated in American archaeology, such ideas would have seemed hopelessly old-fashioned, but now opinion has begun to shift on this score. Childe would certainly be interested in Martin Wobst's (1978) argument that, in addition to generally being restricted to marginal environments, modern hunter-gatherer societies have been transformed, often to a considerable degree, by economic, political, and cultural contacts with neighbouring, more complex societies; hence they cannot be assumed to provide a detailed model of what Palaeolithic hunter-gatherer societies might have been like. Carmel Schrire (1980) has recently made this point specifically with respect to the San (Bushmen) of southern Africa. In North America, there is growing archaeological and ethnohistorical evidence that native societies

inevitably were altered, and sometimes radically so, as a result of European contact prior to the earliest written accounts on which ethnologists traditionally have based their descriptions of these cultures as they might have been prior to European influence (Brasser, 1971). Similar evidence from other parts of the world suggests that in most regions where written records do not antedate European colonisation, ethnology is primarily a study of acculturation under varying degrees of European control, while archaeology alone can reveal the nature of the developmental sequences that produced various pre-literate cultures or establish what those cultures were like prior to their first documentation by Europeans or other literate civilisations.

Childe, with his strongly developed sense of history would have been receptive to the argument that each society, whether there is written documentation for it or not, must be viewed in its own specific historical context. If cultures tend to be inter-dependent and are influenced by the varying relationships that they have with their neighbours, that is to say, if they are open systems with respect to each other as well as in relationship to the natural environment, a changing world system may indeed cause modern hunter-gatherer (often in reality trapping) societies to be different from Palaeolithic ones in many ways: economically, socially, and in terms of their belief systems. One cannot assert dogmatically how much or how little modern hunter-gatherer societies resemble Palaeolithic ones, even when both are located in similar environments. That is a matter for archaeological research to determine. Such an approach would transform the conventional formulation of cultural uniformitarianism, which dates from the 18th century, from being an assumption into being an object of scientific investigation.

Childe would also have seen this argument as strongly supporting the traditional view that the primary goal of archaeology is to understand and explain how human beings have behaved in the past. He would have rejected the idea, promoted to varying degrees on both sides of the Atlantic, that archaeology should become a general science of material culture, dealing with ethnographic as well as archaeological data and seeking to provide generalisations that are valid without any spatial or temporal restrictions (Clarke, 1968; Schiffer, 1976). Such a discipline would be tied very closely to anthropology and would complement social and cognitive anthropology to produce a new tripartite division of labour within that field. Childe would also have rejected, as a regrettable mistaking of a means for an end, the positivist view that the primary goal of archaeology is to explain, not past human behaviour or human history, but rather the variations evident in the archaeological record.

Finally, Childe would have agreed with David Clarke (1979) that we must 'resist an ill-fitting determination to force the patterns of archaeological reasoning within those supposed to hold for other disciplines' (p. 98) and, in particular, to imagine that the range of human behaviour documented archaeologically is no greater than that documented ethnographically (p. 61). American archaeologists have clearly recognised that generalisations about modern hunter-gatherer behaviour cannot be used uncritically to explain the behaviour of earlier forms of hominids (Freeman, 1968: 264). They have also rightly stressed that ethnographic case studies serve to expand the archaeologist's awareness

of alternative explanations for data; they do not themselves constitute explanations (Binford, 1967). It is perhaps the lack in American archaeology of a highly developed sense of history as the scientific study of a world system that provides the context necessary for understanding all archaeological and social science data which has delayed the full realisation of the weaknesses of a neoevolutionary position.

For all these reasons, Childe probably would have regarded ethnoarchaeology as a misdirection of archaeological effort, except where there was obvious cultural continuity between past and present. He would have held the same opinion about simulation studies, especially in instances where essential variables could not be quantitatively measured archaeologically. In both cases, he probably would have felt that time spent conducting such research would better be invested in the direct study of archaeological data.

Society and culture

Childe's greatest strength as an archaeologist was his familiarity with artefacts relating to European prehistory and his mainly intuitive sensitivity that allowed him to spot significant typological variation. Yet, at the end of his career, as he perceived the need for more societal interpretations of archaeological data, his typological skills become a dead-end inhibiting innovation in his own work (Trigger, 1980a: 162–4). Childe, as we have already suggested, would have rejoiced at the progress that has been made in extracting ecological and societal information from archaeological data. Yet I believe that like some Soviet archaeologists, who as a group have had a longer and more intensive experience in attempting to make functional interpretations of archaeological data than have their Western colleagues (Bulkin *et al.*, 1982), he might conclude that all too often such efforts correlate with undue neglect of the formal characteristics of the archaeological record. He would conclude that the long-term vigour and well-being of archaeology depends on maintaining a balance between efforts to use archaeological data to address anthropological or sociological problems relating to prehistoric times and an approach that acknowledges the special nature of the archaeological record. For this reason, he would appreciate the analytical approach of David Clarke, or the corresponding so-called behavioural archaeology of Michael Schiffer, no less than the societal approaches of Colin Renfrew or the American settlement archaeologists.

Both Clarke (1979: 98–100) and Schiffer (1976) have stressed the importance of carefully defining the nature and limitations of archaeological data: how they are transformed by natural processes within an archaeological context and the degree to which artefacts and the spatial settings in which they are found reflect social contexts of manufacture, use, or disposal. An understanding of fundamental archaeological problems of this sort is an essential prerequisite for making progress in the societal interpretation of archaeological data. This is well-exemplified in Stephen Plog's (1980) recent efforts to determine to what degree spatial distributions of different types of pottery within and between archaeological sites in the south-western United States reflect patterns of dis-

posals or trade rather than the distributions of lineages, clans, tribes and other social groupings. This work reveals the premature nature of many efforts to infer social organisation from archaeological data without formally assessing these other factors.

Childe would be especially intrigued, I believe, by recent, apparently quite independent, efforts by Wobst (1977), Tuck (1978), Hodder and others (Blackmore *et al.*, 1979; Bradley and Hodder, 1979) to view material culture as having a significant communicative function in social, political, and economic interaction and to interpret increases in certain forms of stylistic variability within and between societies as a reflection of competition for resources. This competition, which can come about as a result either of ecological stress or a struggle among different social groups to control resources, is seen as promoting the emergence of more clearly defined and sharply bounded interest groups, either in the form of tribal units or class groupings within society. These in turn signal their identity through variations in material culture. Because of this, formal techniques of spatial analysis can be applied to stylistic data to study 'the buildup of social and economic tensions between and within communities' (Hodder, 1979: 453).

Childe would have seen in this development for the first time the possibility for archaeologists to use the Marxist concept of contradictions to interpret evidence of social change in the archaeological record and hence to begin to test the validity of that concept. During his lifetime, Marxist colleagues often complained that he ignored the concept of class conflict, even when he discussed the hierarchical political organisations of the early civilisations, and that he laid too much stress on their functional integration. On occasion, he stated explicitly that the concept of social classes could have no meaning in terms of archaeological data (Childe, 1947b: 60). In part, this may have reflected his belief that archaeologists lacked the information necessary for formulating explanations in terms of human actors and their subjective intentions. In his 'Valediction', he anticipated Binford when he stated that he believed 'foredoomed to failure any attempt to recapture the subjective motives or emotions that inspired the overt acts the results of which alone survive in the archaeological record' (Childe, 1958b: 7). Yet he would almost certainly have seen in these new methods a way to study psychological factors at the level of the group rather than the individual. I suspect that in sensing the possibility of using archaeological data to test, however tentatively, a theory that had long been part of his personal credo, he would have experienced the same mixture of fear and curiosity as he felt when he realised that in the not too distant future radiocarbon dating might pass a definitive judgement on his short chronology. Childe was always prepared, however, to treat Marxism not as a dogma or religious creed but as a way of thought whose truth was to be tested; hence he would have welcomed the possibility that archaeology might play a significant role in doing so.

Cultural change

Childe clearly saw a major goal of archaeology as being to explain change, especially change occurring over long periods of time. He would no doubt be impressed by the great

efforts that have been made in recent years to use archaeological data to formulate and test theories of change, but perhaps bewildered by the diversity of approaches and lack of impact of the results. He would, however, be interested in the widespread abandonment of the idea that was extremely influential in American, and perhaps also in European, archaeology in the 1950s and 1960s, that all internal changes occur slowly and gradually. Many American archaeologists disapproved of Childe's concept of 'revolutions' (Haury, 1962: 117) and devoted much energy to demonstrating that both in the Old and New Worlds food-production and urbanism were the outcome of a process of gradual development stretching over several millennia. The very concept of cultural evolution was equated with gradual transformations and seen as the antithesis of the Marxist concept of revolutions. While Robert Braidwood's and Richard MacNeish's case studies of the processes of domestication in the Near East and Mesoamerica have permanently transformed our understanding of prehistory, a growing number of archaeologists are now considering the possibility that many purely internal changes occur in the form of abrupt discontinuities, either as a result of decisions made by prehistoric peoples on the basis of incomplete information (Adams, 1974: 249) or unusual conjunctions of normally fluctuating variables (Renfrew, 1978). Most of the disputes surrounding René Thom's catastrophe theory concern the degree to which it can be operationally applied, given the limitations of archaeological data, rather than the validity of the underlying concepts as a description of how cultural systems behave. An increasingly detailed understanding of local archaeological sequences also reveals that in the past major discontinuities often were ignored or smoothed over by archaeologists, who underestimated the significance of negative evidence and assumed that conditions remained the same when they did not (cf., e.g., the treatment of Nubian history in Emergy (1965) and Adams (1977)). Diffusion and migration clearly were not the only concepts that formerly were used to discount the importance of internal changes in shaping the archaeological record.

I am convinced that Childe would be appalled by the revival, in recent years, of diverse reductionist explanations of human behaviour, especially those associated with sociobiology and general ecological principles (Hardesty, 1980). These approaches would offend him as a humanist and as someone who believed in Emile Durkheim's principle that social facts must be explained in social terms. Above all he would have objected to these explanations as a Marxist, on the grounds that they deny the possibility of genuine novelty occurring in human affairs as a result of cultural change irreversibly transforming not merely the forms of society but also many of the basic laws that govern human behaviour (Childe, 1947a). Despite statements by Marx concerning the general unity of the sciences, I doubt that Childe would have had much sympathy for the modern view that emergentism and reductionism are not antithetical concepts but 'the obverse of the same basic idea of the fundamental unity of phenomena described at different levels of analysis by different disciplines' (Bindra, 1981: 44). He might have been willing to make an exception of general systems theory, which, while suggesting that there are certain inherent limitations on the degree of variation in any system, is also capable of taking account of major transformations in the structures of social systems.

I believe that Childe would also be appalled by the general decline in a belief in progress that has occurred in archaeology in Western Europe and America over the past 20 years. His own faith in progress was weak enough and he was not the simplistic evolutionist that he was made out to be in Julian Steward's (1955: 122) influential misrepresentation of his work. He not only was a multilineal evolutionist but also believed that, under specific conditions, progress could be blocked indefinitely and civilisations could regress or even destroy themselves (Childe, 1947a). He chided the more doctrinaire Marxists for their naive and, in his opinion, unMarxist faith that the future would necessarily be as they wished it (Childe, 1942a: 11–12; 1947a: 73). Yet he clung rather desperately at times to the belief that archaeology could offer humanity hope of improved conditions in the long run (Childe, 1942a).

Childe would be especially disturbed to note the degree to which modern ecological and cultural materialist approaches study each culture in isolation as a discrete adaptation to a specific environment (Trigger, 1979; Wallace, 1980: 425–6), rather than seeing them as interconnected segments of a broader evolutionary pattern that in modern jargon might be described as a world system changing over time (Wallerstein, 1974). Even more, he would be distressed by cataclysmic views of cultural evolution that picture human beings being driven by forces assumed to be largely beyond their control, such as unchecked population growth, environmental pollution, and the depletion of finite resources, towards an ever more dismal and doom-ridden future (Trigger, 1981). Would Childe have seen in these gloomy views of the past a reflection of the political and economic crises that have been threatening the security of the middle classes in Western society increasingly for almost two decades? Finally, what would he have thought of the recent work of Robert Dunnell (1980), who defines evolution as the study of change rather than progress, espouses biological explanations of cultural change, and denies that either traditional or modern cultural evolutionary formulations qualify as science, theory, or evolution?

Childe viewed scientific knowledge, including knowledge of human behaviour, as a unique precipitate of human experience. For him, true knowledge was that which permitted people to understand their environment in such a fashion that they could manipulate it in a knowledgeable and effective manner. He believed that such knowledge, whatever its origins, comes as a result of diffusion to be shared by ever greater numbers of people and provides the cognitive basis for further technological development. It thus serves to advance mankind's ability to manipulate the environment and by doing so transforms the views that human beings hold of themselves and the universe. Childe also stressed that, because of its objective nature, such knowledge is potentially universally comprehensible not only amongst different human groups but also by archaeologists, however great the cultural and technological gap between them and the people they are studying. He contrasted such objective knowledge with religious beliefs, laws, values, aesthetics, and superstitions, which cease to have meaning when the societies they serve change or cease to exist and which archaeologists therefore have very great difficulty in trying to understand in detail from material remains alone. He did not

believe that his version of the Marxist distinction between true and false consciousness would be obvious to the members of individual functioning societies; it would only become apparent in historical perspective (Childe, 1956).

Childe was correct that no reasonable understanding of human history can ignore these cumulative intellectual achievements. They and the changing awareness that human beings acquire of their environment and of their own nature as a result of them are a reflection of unique human capacities, and the ability of people to transfer such knowledge from one individual or group to another makes cultural change a process that is fundamentally different from biological evolution. Even if we were to destroy ourselves and our planet through misuse of such knowledge, the knowledge itself would constitute the unique achievement of a unique and supremely interesting species. While these characteristics of human behaviour can easily be accounted for within the Marxist framework of materialistic explanation, they provide the basis for an orientation that is simultaneously materialistic and humanistic, rather than simply materialistic as is the case with cultural materialism. The growth of such knowledge can also only be understood in terms of a broad historical framework. This is not provided by a neo-evolutionary approach and is even less apparent in the work of modern cultural materialists. The basically ahistorical nature of both approaches may account for why an interest in the growth of knowledge has been receding in both archaeology and anthropology in recent decades. It is perhaps in this area more than any other that Childe's work offers a broadly-based point of departure for understanding neglected aspects of the archaeological record.

Archaeology as a world system

Today archaeologists throughout the world share a broad range of method and theory concerned with eliciting information from archaeological data. New ideas of this sort, especially techniques of excavation and analysis, have always tended to diffuse rapidly. In recent years, approaches that once were characteristic of a particular tradition of archaeological research have been borrowed or re-invented by other traditions with increasing frequency. For example, societal analysis and use-wear studies are now as characteristic of Western as they are of Soviet archaeology and Soviet archaeologists increasingly are recognising the need for more formal analysis of archaeological data, as well as paying attention to diffusion and migration as processes that are of considerable historical importance. An interest in ecological problems has been growing everywhere over the past several decades. All of these techniques and the information derived from them seem to be part of an emerging international discipline of archaeology into which divergent local schools with their idiosyncratic theoretical orientations and favourite forms of descriptions and interpretation are disappearing (Clarke, 1979: 83–6, 153–5).

Yet whenever archaeologists go beyond trying simply to elicit specific information about chronology, economic organisation, and related topics from archaeological data to become practitioners of a social or historical science concerned with answering significant

questions about human behaviour, this unanimity evaporates. Different political and philosophical orientations and national loyalties lead archaeologists to investigate different problems and determine the kind of evidence that they find persuasive. The Marxist search for a pattern to history that will culminate in the disappearance of class society contrasts with the efforts of positivistically-orientated American archaeologists to establish correlations between individual variables that are often stated to be of predictive value for modern society. The Polish philosopher L. Kolakowski (1976: 229) has described the pretended ethical neutrality of a positivistic approach as a reflection of the values of a culture in which 'technocratic efficiency is considered a supreme virtue'. Nationalists seeking to glorify the history of various elect or repressed peoples also clearly influence the direction of archaeological research and interpretation. It is no accident that archaeologists of European descent, in North America, Australia, Rhodesia, and other colonial situations, often reinforced a static and primitive view of local prehistory during periods when White settlers were actively engaged in dispossessing native peoples of their lands and freedom to manage their own affairs (Trigger, 1980b; Mulvaney, 1981). Nor is it an accident that Chinese archaeologists, both on the mainland and elsewhere, glorify the late prehistoric and protohistoric sequences of northern China as the fountain-head of their national culture; a position that in recent years has drawn a vigorous response from their Vietnamese colleagues. Similar biases, both subtle and unsubtle, bedevil all of the social sciences. Yet, wherever even minimal standards of objectivity prevail, the result is a slow precipitate of facts that gradually narrows the range of possible speculation.

Throughout the early and middle stages of his career Childe had actively used archaeology as a means of combating Eurocentrism, especially as it manifested itself in nationalist trends in German archaeology. Later, perhaps rather idealistically, he argued that the study of prehistory demonstrated the importance of international cooperation and unhindered technological and scholarly communication (Childe, 1958c: 172-3). He did not penetrate beyond these committed positions to consider more objectively and in detail the interrelations between archaeology and the social context in which it is practised. Whether the antihistorical excesses of positivism in American archaeology in recent decades would have led him to undertake a Marxist analysis of the relationship between archaeology and society is a moot point.

We could speculate about Childe's reactions to many other modern trends in archaeology. Quantification played only a minor role in his interpretations of archaeological data, though sometimes he praised it as a means of overcoming personal biases of the researcher (Childe, 1936: 2). He also proposed some quantitative generalisations on the basis of archaeological data, for example, concerning regularities in the size and population of neolithic settlements in Europe (Trigger, 1980a: 150). Likewise, his study of the distribution of megalithic tombs on Rousay should have made him appreciate the usefulness of more rigorous forms of spatial analysis (Renfrew, 1973a: 135-7). Yet, in general, he argued, often unconvincingly, that 'mathematical formulae' were not very useful for interpreting archaeological data, especially from a functional

point of view. I suspect that, like many older archaeologists today who lack a formal training in the physical sciences, Childe would have been torn between admiration for the greater precision in research design and improved insights into the significance of archaeological data resulting from the employment of mathematical approaches and fear that such developments might adversely affect the traditional humanistic orientation of his discipline. It is, of course, possible that, properly used, mathematical approaches might enhance that orientation.

For lack of clues in his writings, I must leave it to those who knew him to speculate what his reactions might have been to other recent developments, such as industrial archaeology and the conservation and rescue archaeology that is establishing itself within the broader context of what has come to be called Cultural Resource Management. Childe might well have favoured the development of industrial archaeology as a means of drawing archaeology and the history of science closer together (Trigger, 1980a: 179).

Childe and the present

I hope that this widely ranging survey of the relevance of Childe's work to the archaeology of the present has made two main points. The first is that, if his early work, which corresponds to the beginning of what in America is called the culture-historical phase of archaeology and which was discussed in detail by Professor Grahame Clark in the first Childe Memorial Lecture, is obsolete, much of his later work deserves the attention that is being paid to it by younger archaeologists. In particular, Childe's view of the centrality of an historical perspective, urged so forcefully at the end of his 'Retrospect' is not the defence of an old-fashioned culture-historical approach, which he himself had long before ceased to find interesting. Instead, it embodies a vital insight into the nature of social science research which accords with disparate recent developments that may mark the beginning of a post-positivistic, post-neoevolutionary trend in American and Western European archaeology. The relevance of historical perspective will increase as archaeologists realise that cultures normally do not exist in isolation but are segments of larger interacting behavioural systems and that ethnologists are not studying pristine cultures but inevitably ones that have been altered to varying degrees as a result of European contact. Because of this, archaeology alone can determine the nature of pre-historic cultures and trace their changes within the context of evolving world systems. Childe also stressed the importance of considering, within this context, the growth of practical knowledge and changing understanding of the human condition as a common heritage of all mankind. This too is something that most archaeologists have lost sight of in recent years. For all of these reasons, the current interest in Childe's later writings, which were little read or understood during his lifetime, is more than a transient fad. Despite the passage of time, these works contain messages that are still worth reading and pondering.

The second point is no less important. Childe enjoyed scandalising those who could be shocked easily by inserting deferential references to Stalin into his public addresses

and prominently displaying a copy of *The Daily Worker* on his desk or under his arm. Yet when it came to promoting ideas that he regarded as being important to archaeology, he took great pains to express them in terms that would win the support of as many of his readers or listeners as possible. Marxist concepts, for example, often were expressed as far as possible not in conventional socialist jargon but in the more familiar and acceptable terminology of British social anthropology. This was done not to deceive his audience but to ensure that ideas were evaluated on their merits, rather than being dismissed out of hand for ideological reasons. Childe hated and opposed German National Socialism, even during the period of the Hitler—Stalin pact, which, unlike many British Communists, he did not support. It is clear from his letters that he felt little sympathy for the hardships suffered by the German people after World War II; instead he regarded these as just retribution for their folly (letter of Childe to Braidwood, 29.5.1946). Yet at a conference on the future of European archaeology that he attended near the end of the war, he not only argued for the need to secure the continuation of archaeological studies in Germany but also defended the archaeologists of the National Socialist period on the grounds that most of them had reported their findings objectively in spite of the conclusions they had attempted to draw from them (Childe, 1945b: 6).

Despite diverse ideological loyalties, archaeologists throughout the world study the same kinds of data and share numerous problems in common. We have already documented the degree to which techniques of archaeological analysis have converged in recent decades. Archaeologists are also united by studying a human past that both diffusion and parallel development have made universal to a considerable degree. They should strive therefore not to let ideological differences prevent them from learning from each other. Childe played an outstanding, if at times apparently one-sided, role in his efforts to promote international contacts and protect the free flow of scholarly information. His reasoned protests against the raising of barriers against such contacts constitute a rich legacy to our profession (Childe, 1958c: 172–3). In a decade when blind political commitments and growing international tensions threaten the advances made during the relatively relaxed period since the 1960s, his views acquire renewed importance. While he perhaps was not always judicious in his pronouncements or his silences, his concern to promote the international welfare of archaeology and science in general and through them the welfare of mankind shines out as a beacon of sanity whenever the dark clouds of bigotry and fanaticism gather.

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Gift Exchange and the Hallstatt Courts

by KARL KROMER

Despite the title given above I wish to deal briefly with several other facts and problems which have occurred to me during my work. I must preface this, though, by saying that I have not found any final solution to any problem. My paper will deal with 'Gift Exchange' but, beyond that, could bear the title: 'Miscellaneous Remarks on the Hallstatt Culture.'

Now let us deal right away with the notion of gift exchange. In contrast to archaeological research at the beginning of the century, from the middle of the century greater emphasis has been given to the economic situation and the social structure in the separate prehistoric periods. To the question of economics belongs trade, particularly foreign trade, dealt with at the symposium by Dr Northover and Dr Alexander.

As far as I know, Kossack was the first to indicate that some of the objects that we can safely regard as coming from foreign regions could be interpreted as exchange gifts. No doubt, gifts have been exchanged throughout the ages: that corresponds to a certain attitude of mind in human relation in general and persists to our days. However, at the time of the Hallstatt culture, gift exchange obviously had a special meaning. Or we can say, the necessity or custom of exchanging precious gifts was pre-eminent and especially practised; this custom brought several economic and political advantages. From the Mediterranean areas about the same time we have reports in some detail that the exchange of presents represented not merely a social duty but also had to follow quite specific rules.

Kossack (1974) has given us an illuminating example of how one was simply 'impossible' without an exchange gift; one was without standing, without property and therefore, without power — all in all, without any political or personal significance. He refers us to an episode on the march homeward of the 10,000 Greeks from Mesopotamia when the survivors had arrived in Thrace and Xenophon was expected to make gifts to the king of the Thracians. However, after the march through Asia Minor, he had, understandably, nothing to hand which could express his merit, his significance and that of his warriors. In this depiction of the social and political situation, we are given an insight into the corresponding usage. Before the meeting of the leading personalities (the king or prince and the leader of the Greeks) helpers went around, who had to discover what the individual guests wanted to give or could give. That was necessary, for even the

host had to have appropriate presents ready. We see, too, the nature of the presents: fine horses, slaves, silver vessels, precious carpets and dresses for the women. All this is indicated only briefly by Xenophon, since his contemporaries were better acquainted with this custom than we are. Xenophon had nothing to give, as we stated, and was at his wits end, especially as he, as the guest of honour, was seated next to the Thracian prince, Seluthes. It would have been outrageous, even scandalous, to make no gift, so, already a little drunk, as he himself reports, he made a gift of himself and his band of warriors as a military force for a limited period to the Thracian prince.

We now have ample evidence that the Hallstatt population in Central Europe had close contact with the world of classical antiquity. But not only did they have contact and knowledge of the attitudes of the upper classes, the aristocracy of the Mediterranean city-states, there is no doubt that they were also at pains to imitate this life-style. As this is generally recognised, I need give only a few examples.

Upper class tombs at the seat of government of a prince were sited along the main approach to the city-like settlement. That is demonstrated to us with particular clarity for example by the little known layout of Burgstall near Oedenburg or Sopron. There are numerous examples showing that such a layout corresponds to the same custom in Etruria. The graves along the Via Appia in Rome show the same custom, too. Originally this was alien to Central Europe. The graves of the great urnfield settlements lie in enclosed burial grounds off the roads leading to the fortress.

Another example: tumuli often had a circle of stones placed around the earth-mound of the grave. This had originally surely only technical significance as support for the mound. During the later Hallstatt period, these arrangements of stones took on the form of a beautiful and regular *krepis* with an outer surface. Zürn (1964) has shown this in the mound at Hirschlanden. These Hallstatt mounds once presented the same appearance as, let us say, the Etruscan tumuli.

In this connection, I should mention here the large scale sculptures which now appear in stone. It may be that these were of wood at an earlier date and have not survived. There is no doubt that we can see here techniques based on models from the Mediterranean region.

The clay-brick wall at Heuneburg can serve as the last brief example. From this kind of masonry and the presence of bastions there can be no doubt that classical military engineering was known in the region of the upper Danube. It was copied and presumably the advice of a builder or architect from the south was available.

I do not doubt, therefore, that there have been personal contacts between the Central European area of the Hallstatt culture and the cities on the Mediterranean coast. But, as we have seen, without an exchange of gifts an agreeable and profitable contact with the rulers, whether kings, tyrants, city governors or senior magistrates, was out of the question. What the visitor from Central Europe brought, we do not know. Maybe this has received too little attention during excavation in the classical stratum. But the counter-offerings are occasionally found to have survived in the Hallstatt sites. They were previously regarded as imported objects and assigned to evidence of foreign trade. They counted as

objects which had changed ownership without relevance to personal or social conditions.

Later on the custom of exchange gifts can be observed quite clearly in the social structure of Celtic tribes. Only by acceptance of a gift was the relationship of dependency established. Counter-gifts were necessary, whether as a true gift or as a form of service or duty. Obviously from this proceeds a generally accepted usage: the lord gave more than the vassal. That this was observed precisely is shown by certain incidents which have been reported. A Celtic prince, who was parsimonious beyond all measure in his gift making, was finally killed by his vassals. It is obvious that his greed had gravely offended against the common usage. Transferred to our problem of the Hallstatt exchange gifts that would mean that the prince had to give more than the guest. Exchange gifts had, therefore, to stand out by their preciousness or rarity or other distinction.

Now I am concerned with finding out which of the many imported objects can be considered exchange gifts: We must however consider that:

1. They were surely not commonplace objects, such as the simple earthenware vessels from the south which accompanied cargoes of wine. Such are the many simple black varnished pieces which were found at Monte Lassois and many other places in Central Europe (see Jacobsthal and Reinecke).
2. As non-exchange gifts can be considered objects which require special manufacture, such as the gigantic cauldron in Vix (Cook, 1979; Gross, 1980). Such an object is not suitable as a gift and something of this size cannot be brought back from a visit to a foreign country.

As an exchange gift could be considered for example the beautiful volute crater found in the Heuneburg (Kimmig, 1964). As a mere makeweight to a wine cargo it seems to me too precious. Nor does it merely serve to pour out the drinks as do many other ceramic makeweights from the south. It is more appropriate to the drinking utensils of the superior warrior caste of the Hallstatt culture. The drink was mixed in the crater as otherwise in the big *situlae*, less with water as was usual in the south than with spices or herbs, as is depicted in the figurative decorations on *situlae* (Kromer, 1980).

This brings us to the drinking customs, which Kossack (1964) has outlined so clearly. I should like to expand on them. I have no doubt that amongst the so-called religious drinking customs there was also a great festive banquet. That is shown by the hooks for boiling meat which are often found in *situlae* with animal bones (Kromer, 1974). They, too, surely belong to the equipment of a warrior of the superior caste. The wine must have been handed round to the accompaniment of some kind of rhythmic music. Here I would cite the so-called 'raps' or 'files' which are not suitable for use as tools, but which are more likely to have served to make rhythmic sounds as part of the sacred magic music which was performed at warrior drinking feasts: Hallstatt tombs 55, 462v, 466, 469 and Linz 49.

Back to the exchange gifts. If the custom of gift exchange was known and practised in Central Europe as well as in the Mediterranean countries we can be sure of another fact. The structure of the Hallstatt population demonstrates an abundance of small

independent areas of sovereignty. Hundreds of fortified settlements, more probably fortified seats of rulers, cover the fertile areas where the economic situation permitted an aristocratic class with its distinctive needs to raise itself above the basic population. Obviously these individual lords were politically independent. There is no evidence of extensive political cohesion and much evidence points to the possibility that they lived in incessant strife and rivalry. The traces of battle on the helmets in the warrior graves in Slovenia indicate this. This is certainly not an instance of equipment being made deliberately useless before being deposited. In the west the levels which show that conflagrations followed each other at short intervals, for instance the layers showing the destruction of the Heuneburg, are evidence that nearly every generation had to reckon with a devastating military event.

It can also be assumed that in the intervening periods of peace visits were exchanged by the war-like lords. Obviously the Hallstatt princes exchanged gifts, in the manner which we know from classical writings. If this occurred in a narrow geographical area, the archaeological sources do not reveal it. If, however, regions were concerned which lay just outside the area of the Hallstatt culture, it is possible to interpret certain groups in the Hallstatt culture whose cultural development did not proceed in the same manner. Two examples can be quoted: one is the so-called chieftain grave at Oss in Northern Brabant.

The chieftain grave at Oss lies in the southern Netherlands, in northern Brabant. I must point out the fact that this site lies outside the territory of the Hallstatt culture although perhaps not far from the limits which reach into Belgium. Nevertheless, Oss lies relatively far from the place where the choicest find from Oss was originally made, the long iron Hallstatt sword.

The work on the find by Holwerda (1934) has been very considerably complemented by Modderman (1964). We can now see equipment in the grave at Oss such as it is believed a Hallstatt lord would possess about 650 BC. The accoutrements are complete. As we shall see, nothing was lacking. The eminence of the warrior is shown by the iron Hallstatt sword. The hilt is splendidly covered with gold foil. To the complete Hallstatt accoutrements belong the bronze utensils for the festive round of drinking with the vassal warriors. Here in Oss there is a bronze situla just under 50 cm high. A Hallstatt prince showed his full dignity by riding on a charger or travelling on his chariot of state. I have often ascertained that the function of the horse in the Hallstatt culture was not for war, as amongst the mounted people of the east. It was a status symbol. On horseback or in his fourwheeled chariot, the feudal Hallstatt lord's war significance was clearly visible. All the utensils he needed such as cheek-pieces and rein ornaments are present in Oss. Modderman has excellently presented the badly conserved objects. There are three sets of snaffles, just the number which was needed for the chariot teams in the find from Kilin in Bohemia and in southern Germany.

Amongst the other objects, fragments of an iron dagger can be identified, hence my dating of 650 BC and not, as Holwerda suggests, 700 BC. In addition, the inventory gives iron knives, a whetstone and other remains. The various large rings are to my knowledge

remnants of ring mail: there are very similar ones in Hallstatt warriors' graves. They were sewn on to a jerkin and made an effective protection against blows with the sword. They were less effective against lances or the flight of a spear. But the main weapon at this early stage was the long cutting sword.

The warrior from Oss was cremated. It could be ascertained from the ashes that he was less (perhaps much less) than 50 years old. He was of slight build and a slight malformation of the upper back bone hampered the mobility of the upper body.

Doubtless we see in Oss the complete equipment of a swordsman. All this cannot possibly have been assembled by trade. The equipment is standardised for a distinguished warrior. But the site lies outside the area of dissemination of Hallstatt culture. Holwerda said in 1934: 'The precious objects must probably have come simultaneously from that Southern Central Europe, brought by one and the same person, who had come far away to our Northern climes'.

How could a leader of the Hallstatt warrior caste come with his whole equipment so far north? Why the situla for the festive round with his men when they were not present? Was he a refugee, an exiled aristocrat from Central Europe?

Let us consider the whole find. This was a funeral by cremation, which was typical of the early period of the Hallstatt culture but also represented the native custom in north Brabant. It was a tumulus and these were common in north Brabant, too, although there they are generally smaller in size. The largest grave considered to be native is about 35 m in diameter. The mound at Oss is larger, being 52 m in diameter. However, the plan and method of burial are wholly identical with the native practice. The only alien element is the nature of the warrior's accoutrements. They are all alien — but I must stress — uniform and complete.

At this point I am forced to conclude that it was not a Hallstatt chieftain who was buried in a foreign land but a native chieftain of north Brabant. The slender warrior obviously had contacts with the Hallstatt sphere. By a personal act he had come into possession of the accoutrements of a feudal Hallstatt warrior. Because of his slight build and his stiff spine which he may have acquired later he would not have been a great warrior. His distinction must have been in another field. He could have received the equipment only from the Bavarian—Württemberg area. An iron sword from Gomadingen-Sternberg has a very similar gold covering on the hilt (Rieth, 1942). Similarly decorated, too, are the swords from the Hallstatt burial grounds. These are the swords from grave numbers 299 and 573 (Kromer, 1959). Naturally these swords, too, were manufactured in the southern German region. The components of horse bits from Oss indicate southern Germany, too.

What is to prevent us from reversing the picture which Holwerda has drawn and which Modderman has summarised as 'an example of cultural contacts between Central Europe and the Southeast part of the Netherlands'? It is not more probable that a respected and enterprising chieftain from what is later called Brabant travelled in the southern German Hallstatt area, remained there for some time and received a complete set of feudal accoutrements as a gift? With these, he was, like other members of the ruling

class, immediately recognisable as 'nobleman'. The accoutrements, the gifts, he then brought home. With them he was also buried, because for his peers all these objects were useless. They were unfamiliar with the customs of the Hallstatt chieftains.

So much for the chieftain's grave in Oss in Brabant. Now for the famous Býč'í skála cave find in Moravia, the second example which I want to quote.

A few words of introduction to Býč'í skála. As with many old, but quite outstanding, finds, there seems to be a curse on the finds from Býč'í skála. Many decades after the excavations in 1882 the extensive material lay in Vienna unresearched. In the mid-sixties I was busy preparing with F. E. Barth and colleagues from Brno in Moravia, a catalogue of the finds. Above all, the important work by F. E. Barth dealing with the parts of the chariots is complete. We are all waiting in great anticipation and with a great deal of interest the forthcoming new ideas and extraordinary results which I know to be of exceptional importance.

In the meantime some daring views have been uttered about the totality of the Býč'í skála finds. The picture given by Nekvasil (1981) that this was an ordinary burial ground, a funerary place for many in a cavern, is certainly not true and I wish most decidedly to oppose it. Putting aside all other problems about the finds, I would like first of all to give a brief description of the situation.

The Stierfels cave, Býč'í skála, is situated approximately 15 km to the north of Brno in Czechoslovakia. It is made up of a multi-tunnel cave system in the area of the Moravian Karst country. In the central part of the cave were palaeolithic finds. We are, however, interested in the various finds found at the entrance of the cave (Wankel, 1871, 1872, 1882). In 1872 students accidentally came across the famous bull sculpture (Beninger, 1932-3). It had been the handle of a bronze basin similar to the one we know from the burial ground at Hallstatt. In the same year Dr H. Wankel continued the excavations and a somewhat sentimental reconstruction of the excavation survives to this day. It depicts Wankel's ideas (Poulik, 1956). Today our conception is different in some ways.

A nobleman is seen to be lying on a magnificent wagon, around him are men, women, horses and other domestic animals as well as a large number of valuable bronze containers, pieces of jewellery and pottery. Everything is waiting to be set alight.

F. E. Barth was able to show that several wagons belong to the inventory of the grave. We shall see the residue of iron and bronze objects which survived the fire.

Undoubtedly Dr Wankel's reconstruction gives a romantic distortion of the actual situation, but many details are correct and can be checked. For example, we can see the head of a beheaded man in a bronze basin. In fact, there is a skull with a green patina amongst the skeletal remains and in this instance the particular observation of Wankel could be right.

The many unburnt human remains may serve as an indication that the site may have been a sacrificial site, perhaps for one uniquely large offering. Anthropological opinion provisionally allows for at least 41 individuals. It is of importance that there are approximately the same number of men as that of women and that the age groups are also represented by a similar ratio.

The relationship between male and female individuals can be seen, if we avoid our modern thinking of categories of juveniles and adults. This is a modern division and we have no need to apply these age groupings.

We have in the Býččí skála cave:

Two small children up to six or seven years of age. Five older children from six/ seven to 12/14 years of age. No males, three females, juveniles from 14 to 20 years of age. Fourteen males, 11 females, adults from 20 to 40 years of age. This means 14 males, 14 females juveniles and adults together. One male, one female, middle aged from 40 to 60 years of age and no male, no female, senile, over 60 years of age.

And now to the famous skull cups (Krenn, 1929). They seem to point to two males and two female adults. The event which created the find in the Býččí skála cave was purely local. The ceramic objects all have unquestionably an indigenous flavour. Notions that this is an instance of an incursion by a foreign people, such as the Scythians from southern Russia, who performed a great sacrificial ceremony, are assuredly not true.

But a small number of the finds from the Býččí skála cave indicate connections with the western Hallstatt world. These objects could have arrived in the eastern Hallstatt sphere of influence through trade, but more probably, I believe, for purposes of hospitality and giving of presents. Again, they are accoutrements of the superior warrior classes, things essential for the lifestyle of the aristocratic class.

The weapons should be mentioned first. The Býččí skála find belongs to the late stage of the Hallstatt culture, and this is demonstrated by the antenna daggers which were found there. These western style weapons have been found only in the Býččí skála cave. They are antenna daggers which were certainly made only in the western Hallstatt area and a great number of similar pieces from southern Germany could be quoted.

In contrast to the long iron sword of the earlier phase, these daggers were scarcely appropriate for battle. They are undoubtedly ornamental weapons, most precious objects. Are not such treasures suitable as gifts? Is not a host particularly honoured by just such a valuable weapon?

Other important things are the chariots from Býččí skála. As long as Barth's research remains unpublished we can only speak in a general way about it, but it does seem certain that there was not just a single chariot, as Dr Wankel supposed during the excavation. Quite certainly remains of more than one chariot were found. These were chariots whose wheels were covered with bronze foil and there were chariots with iron-covered wheels.

The most famous one has become known through special exhibitions. It was probably a two-wheeled vehicle. The spokes are covered with ribbed sheaths of bronze, which create the optical illusion of a spiral when the wheel revolves. We tried to find out at what speed the maximum illusion was created as this would tell us at what speed the wagon had been driven. It was at least about 10 or 12 miles an hour. We have depictions of such chariots on the figurative decorations of situlae. They are single axle vehicles with

an artistically decorated vehicle body and were driven by a single driver. The gold jewellery that was found here also came from the west. The brow-bands or large ear rings, if one wishes to differentiate, correspond closely to the gold of the western Hallstatt because of their embossed decorations.

Let us consider the find of Býčů skála as a whole and see whether it yields any solution to the problem of exchange presents. This is not a case of a burial site as some Moravian colleagues would like to believe. It is quite certainly a single unified deposit. Let us ignore the fact that there are so many bodies. This was certainly an eastern custom, which was completely alien to Hallstatt man. We can be assured that the whole find originates from a single great sacrifice of humans, animals and possessions.

But, on the other hand, the whole find can be considered as a grave, not as romantic as it was depicted in Wankel's day, but a princely grave with uncommonly rich accessories. Thanks to the prince's tomb at Hochdorf near Asperg which has come down to us intact, we know how extraordinarily richly equipped were princes' tombs at that time. Compared with other princely burials, only the animal accessories in the Býčů skála cave are striking. We have not been able to observe the addition of horses in any other grave from the Hallstatt culture. However, if we remember again the eastern influence shown by the human component, that could explain it.

The most important accoutrements are alien to the eastern Hallstatt area, but they, too, form together a complete group of objects: weapons, chariots, ornaments, anything a leader needs in the late Hallstatt period to show his social position. In this Moravian cave numerous objects are particularly richly represented, above all the chariots. There were more than one and all from western workshops.

We must ask whether all that can be explained by 'trade'. I believe not. Trade would have left finds outside Býčů skála, that is, outside Moravia too. Chariots of this kind were very fragile and, correspondingly, costly, but we have not a single piece of evidence for trade with the western area in the whole of the eastern Hallstatt region. These western objects must have arrived in Býčů skála in some other way.

Is it so out of the way to assume a personal contact, a personal relationship between a prince of the eastern Hallstatt area and a princely court in the western area? He had it, because he was widely travelled. He alone had this connection. In any other complex of finds from eastern regions at that time, western objects are lacking.

The last word has not been said about this. We need the complete publication of results or at least the ones regarding the chariot parts from F. E. Barth, which are ready for publication.

However, with the concept of gift exchange, we can possibly shed some new light on the old finds.

Let us turn back to gift exchange in general. Until now, we have not progressed much beyond sheer speculation. We can indicate possibilities, but not proof. It seems to me that the way the presence of alien objects in some sites has been interpreted until now has been too general and too limited. There has been talk of trade. Certainly trade played an enormous role during the Early Iron Age, but equally certainly not everything

can be explained by trade contacts. The expression 'cultural contacts' satisfies me even less. It is often the vaguest expression of our knowledge and says as good as nothing. So, I believe, we can explain more if we bear in mind in future the custom and practice of gift exchange. I am sure that this goes beyond the generalisation 'cultural contacts' and can provide a deeper insight, perhaps even into the personal relations of the Hallstatt princes.

Abstract

Specialised forms of gift exchanges were a feature of Mediterranean societies of the first millennium BC, and there is much evidence that the Hallstatt population of central Europe was in close contact with these societies. Some examples are cited to show that the leading men of the Hallstatt communities were at pains to imitate the customs of the Mediterranean aristocracies. There was probably personal contact between representatives of each, with consequent gift exchange, of which we can only identify one side as yet in e.g. the Heuneburg volute crater. Exchange of gifts between neighbouring Hallstatt princes cannot be recognised archaeologically, but two sites outside the Hallstatt area, the chieftain's grave at Oss in N. Brabant and the Býčů skála site in Moravia, which it is argued also represents a princely burial, do contain splendid weapons, armour and chariots of Hallstatt type which seem more likely to represent gift exchange than ordinary trade.

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Prehistoric Saltmining at Hallstatt

by F. E. BARTH*

In 1846, when Johann Georg Ramsauer discovered the cemetery of rich graves at Hallstatt — which eventually gave its name to a whole epoch — nobody doubted that the immense wealth documented by the finds was derived from ancient salt-mining (Kromer, 1959). Salt must have been a much prized product since all this splendour could be acquired in exchange for it. According to Bloch (1970), 'In a man's life salt plays a part whose dramatic power is veiled by its everyday familiarity.' Hunger for salt is as tormenting as thirst for water, and one can starve to death and perish for lack of salt as one can die of thirst for lack of water. Yet man's daily need of salt is met with only two grams. There are ethnological examples which demonstrate ways of meeting this demand other than by the industrial production of salt: for instance by using blood or urine, by eating soil, burning plants and consuming the ashes and the like. Hence the beginnings of the industrial production of salt must have had other causes. Hundt (1973) has suggested that a change in climate could have made the traditional method of preservation by drying impossible, and that this may have stimulated the practice of salting and smoking, both methods which require salt. At all events, after the beginning of the last millennium BC a sudden growth in the demand for salt in Europe must be assumed. At that time, in many areas where salt was found, salt production was intensified on a large scale. In many cases salt-springs — that is to say brine welling up out of the ground — provided an available source of raw material. The salt deposits of the Alps, however, had to be mined.

The salt deposits of the Northern Alps are located in the limestone areas near Maria-Zell, St Gallen, Admont, Liezen, Bad Aussee, Bad Ischl, Hallstatt, Hallein, Berchtesgaden and Hall in the Tyrol. The salt deposit itself does not consist of pure rock salt; it is the so-called *Haselgebirge* containing a large amount of impurities such as clay, marl, anhydrite, and gypsum. Rock salt is only found in salt-zones of varying size, the so-called *Kernstreichen*. The high content of impermeable clay in the *Haselgebirge* conserves the salt deposits: after superficial leaching an insoluble sediment of de-salted clay is formed and this effectively seals off the salt dome. *Haselgebirge* is a tough mineral, but very easily moulded and susceptible to pressure. Therefore any cavity formed in the mountain must be timbered if it is not to close up again within a short time.

Today's salt-mine at Hallstatt has been in existence since 1311. In that year, according to the documents, the new mine was 'raised from the green turf' by the German

* Translated by Christine Geyer-Cschradt.

Queen Elizabeth. Her bridal fief was the 'Salzkammergut' (as the region to the north of Hallstatt is called). To enable the systematic mining of the salt deposit, the whole mountain was traversed by a system of horizontal tunnels and divided into levels of working. Since the beginning of the historic period of salt-mining the leaching method of salt extraction has been followed. A small chamber is hewn out between two levels. This can be entered from the upper level by way of a shaft, and it is linked to the lower level by a watertight duct. Then the chamber, called *Laugwerk* (elution chamber) is flooded with fresh water up to the ceiling. This water is left until it is saturated with salt and no more will dissolve. Primarily the salt is leached from the ceiling, with the impurities sinking to the bottom and forming the so-called *Werkslaist*, the insoluble sediments. The saturated brine is transported to the boiling shed via ducts through the lower level, and there salt is produced by evaporation. In several levels of the present day salt-mine at Hallstatt traces of early man's presence in the mountain have been discovered. The miners have interpreted these traces quite correctly without, of course, having any idea of their true age. They have called them *Heidengebirge* (heathen's rocks). As early as 1713, a chronicle by Riezinger (1713) about Hallstatt's salt-mine mentioned 'all kinds of man-made things in the salted rocks' as evidence for the existence of salt-mining in Hallstatt long before the beginning of the historic period of salt production. But information was only discovered by chance until Ramsauer with his inquiring mind was no longer satisfied with such accidental discoveries. As early as 1849 he had the *Kernverwässerungswerk* systematically explored for finds and thus carried out the first well-planned programme of archaeological research underground (Ramsauer, 1850). Indeed, soon after the discovery of the Hallstatt cemetery, public interest in local antiquities was aroused and a critical discussion started about the 'ancient man' of the mines. By 1851, Unger had already produced a paper on 'Plant Fragments found in Hallstatt's Salt-mine.' In 1882 von Hochstätter published a report on 'an ancient Celtic mine'. This is a printed version of a report by the Hallstatt mine management to the Ministry of Finance covering an open mine chamber discovered in 1880 in the Appoldwerk and explored by narrow tunnels. This report and the findings were given to the Museum of Natural History in Vienna. J. Stapf and B. Hutter, two employees in the salt-works, anticipated many of the results of more recent research in this report. Thus the division of the prehistoric sites into three groups dates back to them. To illustrate their report, Stapf and Hutter had magnificent maps drawn. In succeeding years employees of the Hallstatt mines continued to deserve credit for their research on ancient salt-mining. First, A. Aigner (1903), then G. Langer (1936), and finally O. Schauburger (1960), have attempted to review and assess the current state of research from the miner's point of view. From 1925 F. Morton was working at Hallstatt. Conscientiously he collected all the finds from the mountain and from time to time produced short reports on them (1928, 1928a, 1939, 1942, 1949, 1957). An excavation by Mahr and Morton in the Grünerwerk in 1927 represented a promising start to scientific fieldwork underground (Mahr, 1927). Unfortunately, Mahr left Austria before the results could be published. This enterprise at the Hallstatt salt-mine was not followed up for a long time. It was not until 1960 that the long-standing co-operation

PREHISTORIC SALTMINING AT HALLSTATT

between the Hallstatt salt-mine and the Museum of Natural History in Vienna was resumed at Karl Kromer's instigation (Kromer, 1960, 1966). Since 1960 the Prehistoric Department of the Museum has conducted excavations in the Hallstatt salt-works each year.

The places where *Heidengebirge* are discovered are situated at an altitude between 1146 and 880 m in the area of the present-day mine tunnels named Ferdinand, Tollinger, Karl, Katharina Theresia, Leopold, Josef, Christina, Maria Theresia and Elisabeth. Schauberger (1960) has taken the trouble to map each documented site. This praiseworthy work has provided the essential basis for any further research. When projected on to a horizontal plane, the sites show a distinct concentration into three groups which Schauberger called the Eastern, Western and Northern group. He also tried to find a possible or probable connection between the findspots, or in other words to reconstruct the supposed course of the ancient tunnels. He started from the assumption that neighbouring sites should be connected. By this means he produced a confusing system of tunnels without any underlying principle. This result would agree with Mahr's opinion (1925: 29), who wrote about 'toilsome digging', but in my opinion the great vertical and horizontal expansion of the prehistoric mines unmistakably implies regulated mining, directed of course by a distinct plan. On this assumption one can try to examine the individual sites in the mountain and see whether they fit into various different systems. Close spatial proximity then need not always imply a direct connection between two points. Beginning with the Eastern group, a tendency to a more or less horizontal system of galleries may be observed (Fig. 1). There are definite indications for this. The exposure in the Stügerwerk, the only attested ancient exploitation cavern that is still open



Fig. 1 Eastern Group method of working
(from: *Die Hallstattkultur, Katalog zur Ausstellung Steyr, 1980*)

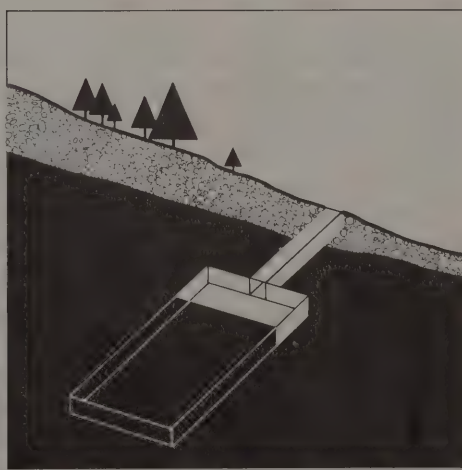


Fig. 2 Northern Group method of working
(from: *Die Hallstattkultur, Katalog zur Ausstellung Steyr, 1980*)

today, has an almost horizontal ceiling approximately 35 m long (Barth, 1982). The 1899 description of the exposure in the Enderwerk reads: 'The gallery is cut into the rock without any shuttering, like a tube about 2 m wide and 1.5 m high, and it continues almost horizontal above the ceiling of the elution chamber (Barth, 1972). Unfortunately the direction of that gallery is not mentioned in the mining chronicle. Thus in both cases where prehistoric galleries in the area of the Eastern group are — or were — open, a more or less horizontal arrangement is indicated. Therefore it seemed worthwhile classifying the sites in this area according to the level and examining the possibility of horizontal connections. Three exploitation galleries, one above the other, are suggested. The uppermost is at the level of the tunnel called Josef, and five prehistoric sites can be attributed to it. The middle one lies between the Josef and Christina tunnels with six sites belonging to it, and the lowest between the Christina and Maria Theresia tunnels. This can be associated with three sites only. It must, however, be borne in mind that one of the three is the Enderwerk, where a horizontal arrangement is clearly attested. It is noteworthy that each of the three tunnels bends westwards, running approximately parallel to a mass of dead rock, the so-called *Steinbergscholle*. I am inclined to believe that the curvature of the gallery is dictated by the inner structure of the salt deposit, the *Kernstreichen*.

This new system for the Eastern group does not take into account the three sites in the Maria Theresia tunnel and the two sites in the Elisabeth main tunnel, because they are too few to form the basis for any further conclusion without additional evidence. It is just possible that these sites belong to inclined adits, which must be presupposed in my system of horizontal exploitation as well. But as I have said before, there is not enough evidence yet to permit definite conclusions.

I have not yet mentioned the six sites in the horizon of the Christina tunnel because apparently they do not fit in with this pattern. The gallery in the suggested reconstruction runs with a slight incline from the south-west to the north-east, thereby aiming vertically and horizontally in the direction of the Iblwerk in the Northern group. It is noteworthy in this connection that in the Iblwerk a partially accessible chamber has been preserved. It is of a kind which, to my mind, from its construction, can only form part of a system of horizontal galleries. To all appearances this site belongs with the Eastern group.

We have a quite different impression when we try to interpret the sites in Schauburger's Northern group (Fig. 2). The Grünerwerk (Fig. 3) is an important site to help us solve this problem (Kromer, 1961). In 1911 there were extensive roof-falls, laying bare prehistoric mining activity. The chart shows this as an inclined shaft orientated at 304° and with a gradient of 35° . Its width was reported as 12 m, its height as 1 m. In 1926 another exposure to the west of this was laid bare during elution operations. As I mentioned before, Mahr and Morton excavated at this point in 1927 (Mahr, 1927). The bottom end of an inclined shaft was laid bare: its orientation was measured as 307° , the gradient is $30-35^\circ$. Thus we are confronted with two parallel sloping shafts representing a gradual incline. If we try to fit the sites around the Grünerwerk with this interpretation we find that they fit. The gradient is kept steady, the direction changes only

PREHISTORIC SALTMINING AT HALLSTATT

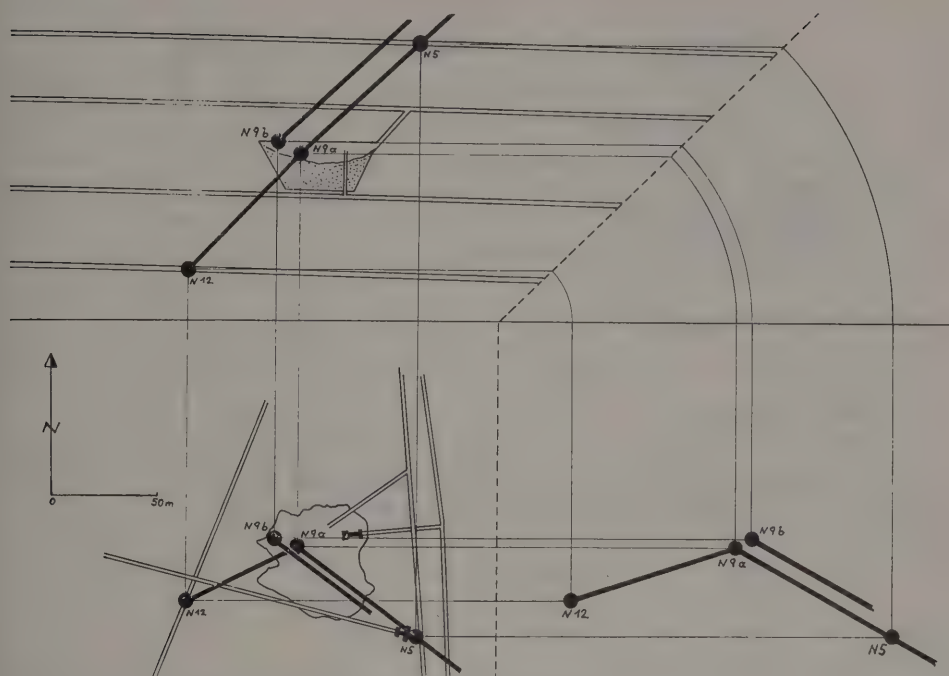


Fig. 3 Grünertwerk system: N 5 lateral Lobkowitz, N 9a Grünertwerk 1911, N 9b Grünertwerk 1926, N 12 lateral Colloredo (after Schaubberger, 1960).

gradually, turning slightly to the south. The advantage of this system of parallel inclined shafts is obvious: connections between the horizons would improve the circulation of air and prevent it becoming foul. Obviously, the other sites in the Northern group, which because of their proximity must constitute a unit, had also to be tested to see whether they fitted into a system with two parallel inclined shafts. The results are not so clear-cut, but the same principle can be inferred by comparison with the disposition at the Grünertwerk (Fig. 4). The two inclined shafts are not as uniform in direction and incline much more steeply than those in the Grünertwerk system. The inclined shaft being exploited penetrates the Appoldwerk and then runs down directly to the lateral Rosa von Seeau. The upper inclined shaft runs on from the Pohadschwerk through the lateral Lahnsteiner and through the Flechnerwerk to the lateral Stampfer. A further difference from the Grünertwerk system lies in the width of the upper inclined shaft, which is about 17 m wide and thus cannot have served for ventilation purposes only.

I am sorry to say that there is not much to report about the last of the three groups, i.e. the Western group. No finds have been preserved and none of the sites is accessible today.

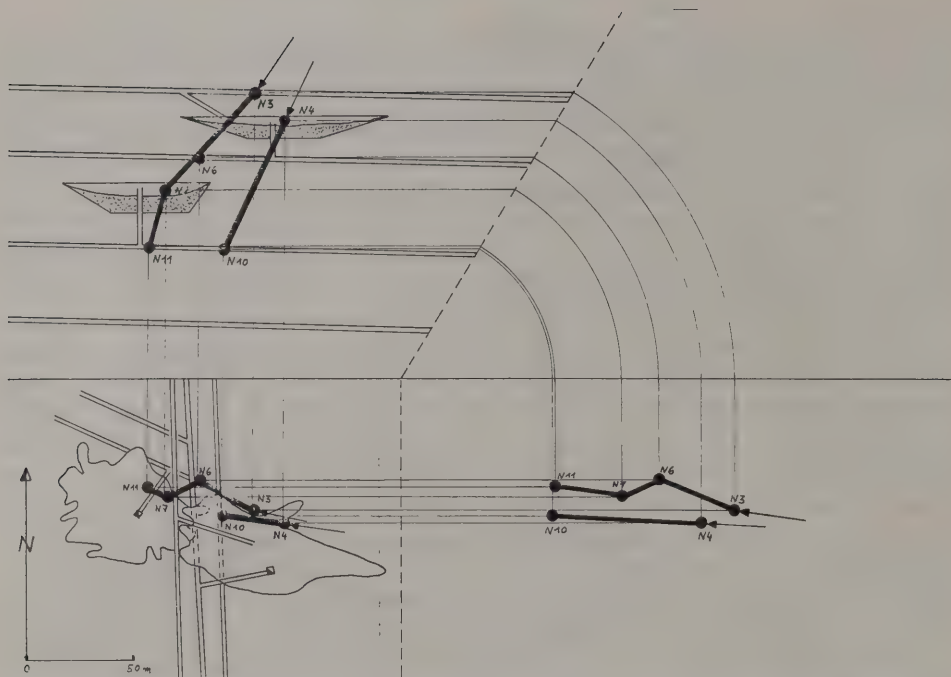


Fig. 4 Appoldwerk system: N 3 Pohadschwerk, N 4 Appoldwerk, N 6 lateral Lahnsteiner, N 7 Flechnerwerk, N 10 lateral Rosa von Seeau, N 11 lateral Stampfer (after Schauburger, 1960).

One of the big problems connected with the Hallstatt salt-mine is chronology, meaning both absolute and relative chronology. It has been assumed that the Western, Northern and Eastern groups succeeded one another chronologically in that order and that they were in operation for the relatively short period of 500 years, that is to say during the period when the cemetery was in use. Now, however, it has been possible to classify the finds from the mountain into a typological scheme (Fig. 5) which provides the basis for a new chronological interpretation. Regrettably, the Western group has to be excluded, again for lack of material. The typological differences between the Eastern and Northern groups are as follows (Barth, 1973):

1. Different systems of mine organisation which I have just described in detail.
2. Probably connected with this, different techniques of mining leaving different patterns of grooves in the rock. In the Eastern group heart-shaped reliefs are found on the rock-face; these were first discovered in the Stügerwerk and were satisfactorily explained by Morton (1949). The prehistoric miner was not content with the fragments (*Hauklein*) he got when merely chipping away at the rock. He cut a heart-shaped groove into the rock and then separated the blocks in between with heavy blows, possibly using a wedge. Halves of such a heart-

PREHISTORIC SALTMINING AT HALLSTATT

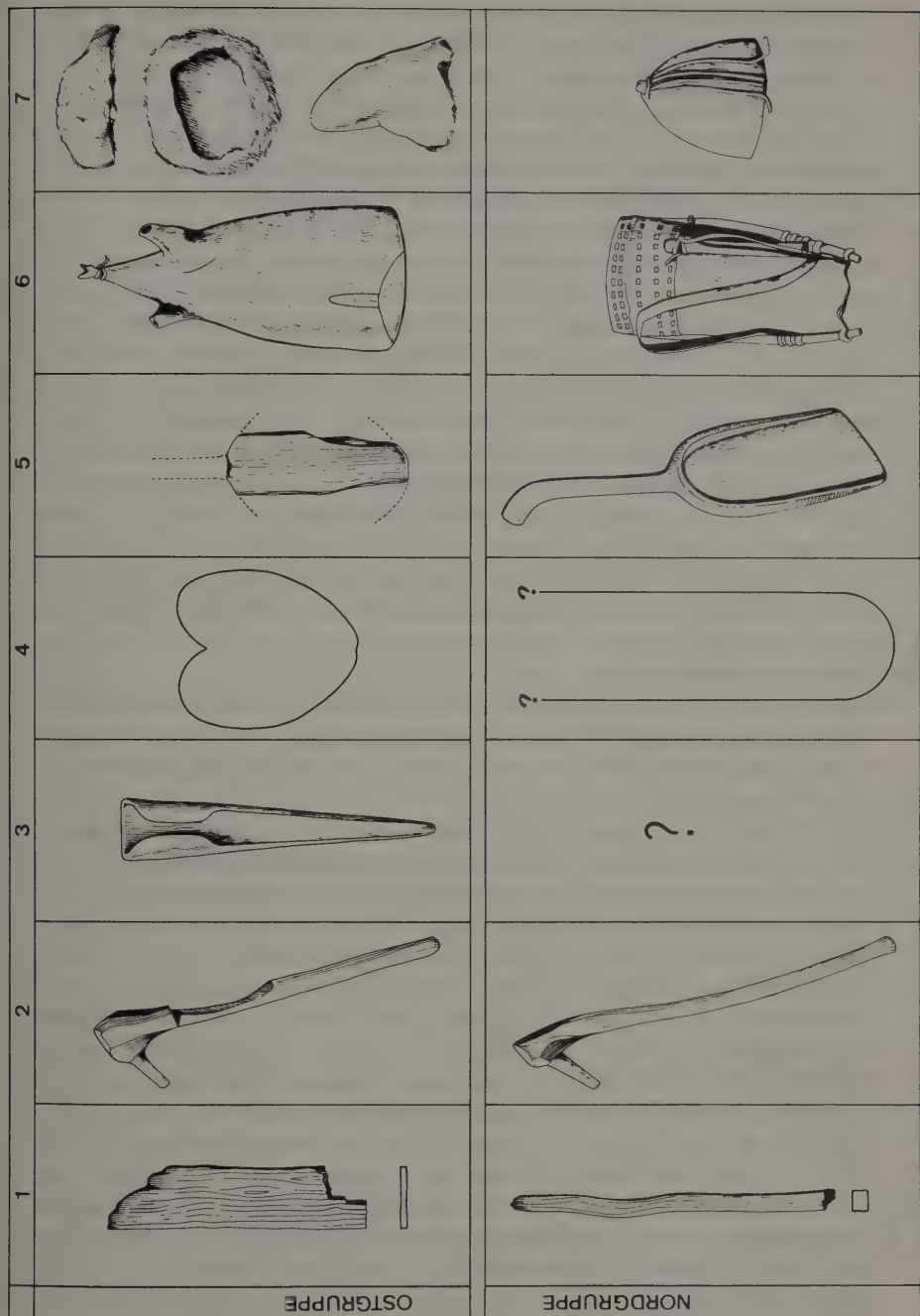


Fig. 5
Typology of prehistoric finds from the Salzbergwerk, Hallstatt (from: *Mitteilungen der Anthropologischen Gesellschaft in Wien*, 102, 1973, 26–30).

shaped block, clearly the trade-mark of the ancient Hallstatt miner, were also found in the Stügerwerk (Barth, 1982). Similar grooves were discovered in the Katharina von Edlersberg-Werk in 1968 (Barth, 1970).

In the Northern group two parallel grooves, about 40 cm apart and advancing in the direction of the strokes, served the same purpose. In 1967 such grooves were discovered in the Grünerwerk, but could not be recorded properly because of the unfavourable working conditions (Barth, 1970).

3. Different tools were used for mining, as is shown by the shafts (Barth, 1967). Knee-shaped hafts with a thickened, club-like head and a marked tapering of the handle in the front third are known only from the Eastern group. The profile in the front section is trapezoidal and then develops into the circular profile of the handle. This tapering made the handle elastic and resilient. When merely striking blows this proves to be a disadvantage, the blows losing quite a lot of their striking force. The tapering makes sense only if we assume that a mallet plus pick technique was employed, similar to that used in the historic period. The hafted tool is merely held against the rock and then struck with a mallet. As a matter of fact, all the hafts of this kind are worn and frayed at the crown and moreover suitable mallets have also been found. The tool that fits this kind of shaft is the wing-ended pick. In the Northern group, hafts have long, rather thin handles, a small cylindrical head and wide, flat shaft-tongues. Thus both the shafted tools and the mining method must have been different from those of the Eastern group.
4. Differences in methods of illumination have repeatedly been pointed out in the literature. In the Eastern group wide, flat spills of spruce or fir were used for torches; those of the Northern group have a square section. The latter were also burned in bundles.
5. The shovels in the Eastern group are oar-shaped with a long blade. In the Northern group shovels were preferred with a large, carefully modelled blade and a round shaft, with the end usually curved to form a bow-shaped handle.
6. Differences between the carrying-sacks are very obvious. In the Eastern group a plain animal skin bag was used (Barth, 1972). The back part is kept as long as possible, with the belly part completed by two gussets. The bottom of the sack was stitched up with a so-called saddler's seam. This is made with two needles which are pushed through two corresponding holes on opposite sides. The front leg stumps were stitched up by the same technique. These stumps served as handles, as is shown by the arrangement of numerous tears. The neck was used as a mouth and was tied with bast. In the Northern group carefully sewn cowhide sacks were used, stiffened with two wooden strips secured with decorative straps (Morton, 1939). For carrying, this kind of sack was equipped with a shoulder strap whose ends were fastened to the lower part of the sack and slung over the shoulder. At the top of the sack a short stick was fastened which went over the other shoulder and must have been held on to during transport.

To empty the sack it was necessary only to let go of the stick and the sack would tip to one side. Then, with a swing, the empty sack would be shouldered again. This means of transport clearly points to the processing of salt chips in the Northern group. However, compacted chippings have not been found at any of the sites in the Northern group.

7. Differences in dress appear to have existed. Berets have been found in the Eastern group more than once. The edge of a circular piece of hide was pleated, then a strap was pulled through (Barth, 1972). Thus it was possible to regulate the width according to head-size. In one instance a pointed cap of hide is documented (Kromer, 1963; Tafel 69). The single cap found in the area of the Northern group is different: it is sewn up from triangular pieces of hide and decorated with tassel-like leather straps (Morton, 1942).

It is obvious that this typology of the objects from the Hallstatt salt-mine that I have just outlined must be accepted as very hypothetical. This must be so because of the comparatively small number of the finds. Some of the suggested typological groups are based on no more than a single object. The number of artefacts is adequate only for the torches and the knee-shaped wooden hafts. The chances of discovery at the site could be enough to explain the other groups of objects. Nevertheless, I consider my hypothesis valid because first of all it confirms the marked typographical distribution of the groups and secondly the general nature of each of the two proposed groups is so consistent. I am especially concerned with the methods of fashioning hides. The haversacks and caps of the Northern group type both show the same careful workmanship and the urge to decorate which cannot be observed on comparable objects from the Eastern group. Here we get the impression of an attempt to minimise effort and achieve maximum utility.

Any typological structure is futile if it is not considered as a mirror of historic conditions and events. There have been several attempts to work out the chronological position of the ancient salt-mine at Hallstatt. Modern research has assumed a connection between the salt-mine and the cemetery and consequently has limited the mining period to 500 years at most. The relative chronological sequence has been thought to run from the Western to the Northern and then to the Eastern group. A different conclusion results, however, when my typological pattern is considered. Looking for comparative finds, it becomes clear that only the Eastern group of the Hallstatt salt-mine can to some extent be paralleled with the cemetery. The chronological range extends from the twilight of the Urnfield Culture to the culmination of Situla art. The mining tool of the Eastern group, the pick, must be considered a survival of the Urnfielders. It appears in Urnfield hoards of south-east Europe and there too it is associated with mining — compare e.g. the hoard of Felsőjvár, Hungary (Holste, 1951: 24, Tafel 45, 30). This late Bronze Age tradition can also be seen in the cemetery. Fragments of peasant smocks found in the Hallstatt mine provide a possible link with Situla art. Berets and peaked caps are among the most frequently worn articles of clothing to judge by the figures depicted (Lucke and Frey, 1962). Consequently, only the Eastern group can be linked typologically with the cemetery.

Unless we are prepared to accept the rather unlikely hypothesis of a second set of miners working in the Northern group at the same time, but with different tools, then the Northern group must be recognised as either earlier or later than the Eastern group. Schauburger has collected samples of wood from every accessible exposure in the prehistoric mines and has had them radiocarbon dated at the Institute for Radiology in Vienna. The results have been surprising (Barth *et al.*, 1975). So far, there are 10 dates available from the area of the Eastern group, all of them fall between 700 and 200 BC. This result is in agreement with the conclusions we reached by typological methods. It confirms the idea that only the Eastern group of the prehistoric mine can be considered contemporary with the cemetery. Dendrochronological dates, for which we are indebted to E. Hollstein, have recently confirmed this absolute dating of the Eastern group (Hollstein, 1974, confirmed in Hollstein, 1980). Two fir trunks examined by him were, in all probability, felled in 656 BC, a spruce tree in 660 BC. These dates have been determined by dendrochronological bridging and are among the earliest in Europe so far. About 50 samples from the Hallstatt salt-mine are now being analysed by Dr Hollstein; it is to be hoped that further exciting results will follow. The four existing radiocarbon analyses from the area of the Northern group were dated between 1000 and 700 BC. In fact, the Northern group does seem to be earlier than the Eastern group, but much earlier than assumed previously. A further argument for the early dating of the Northern group is, in my opinion, provided by the galleries in the mine. We are confronted with a system of shafts, which evidently reached great depths, and which were very wide but remarkably low. This system involves great operating difficulties and is not at all necessary with deposits like those at Hallstatt. This sort of gallery is only productive when working a thin layer of stratum, when the mineral lies in the mountain like a sheet of paper. In this case the aim is to dig as deep as possible, as wide as one chooses, and to keep the access as low as possible. I think, therefore, that we can assume that the mine operators learned their technique while mining copper, then in the late Bronze Age they came to Hallstatt and started salt-mining without bothering to adapt their mining technique to the nature of the deposit.

The chronology of the Western group still presents great difficulties because of the almost total absence of finds. Nevertheless, a short time ago some objects previously attributed to a site in the Eastern group were successfully identified as belonging to the Western group (Barth, 1973). This re-classification was based on Isidor Engl's water-colours. Among the objects were two wooden shovels, from which samples for radiocarbon analysis were taken. One shovel was found to date from 100 BC, the other from 100 AD. In fact, the Western group is situated close to a well-known Late La Tène site in the area of the upper Dammwiese (Morton, 1931). This is an extensive settlement which must certainly be linked with salt production. An involvement with the exploitation of salt-springs has frequently been suggested for it; but in the late 30s Morton excavated the mouth of a tunnel there (Morton, 1964). Regrettably, the results of the excavation have never been published. It is certain, however, that the settlers at the Dammwiese were engaged in salt-mining whether or not they were also involved in processing

brine. In this connection only the Western group can be taken into consideration.

The three groups of the prehistoric salt-mine at Hallstatt, then, are to be seen as three chronologically successive centres of exploitation. We are faced with the question why each group made a fresh start, for none of the three areas was exhausted. The reason for abandoning the Northern group must be sought beyond Hallstatt itself, because the transition from the Northern to the Eastern group corresponds chronologically with a historical break: the change from the Urnfield to the Hallstatt culture. In fact, the fine sediments which partly fill the cavities in the Northern group seem to indicate a gradual falling into decay. These sediments, which can be found at every site that is still accessible in the Northern group, are a consequence of the shut-down, not its cause. Just the opposite holds true for the Eastern group. The filling material there consists of soil mixed with gravel, rootstocks, grass, etc. as is typical for mudflows. Thus the reason for ending the Eastern group seems to have been a disastrous landslide which devastated the entire high valley and flooded the mine (Barth, 1976). The cemetery, situated on the slope, was spared the havoc. This theory would explain the significant lack of finds outside the cemetery. The layers with finds either slid away or were covered by mud many metres deep. The point of time suggested both by the end of the cemetery and by radiocarbon dates for the Eastern group would be a middle phase of the later Iron Age. The latest mine structures of the Eastern group — Edlersbergwerk and Christina gallery, i.e. opencast inclined adits — appear to be unsuccessful trials for new areas of exploitation in the aftermath of the catastrophe. In the 2nd century BC the works were moved to the Dammwiese, which was safe from such dangers.

Abstract

The significance of the development of salt-mining in the Austrian Alps during the first millennium BC is briefly discussed. The nature of the deposits, and the method of working them, is described, and the main stages in the discovery of the ancient workings are outlined. Three topographically distinct groups of workings are then distinguished, the Eastern, Northern and Western. The Eastern group is characterised by more or less horizontal workings, the Northern by a system of sloping shafts. No information is available about the Western group. Differences in equipment reinforce this contrast in working methods between the Eastern and Northern group. Radiocarbon dates indicate that the Eastern group only is contemporary with the Hallstatt cemetery, the Northern group being earlier (late Bronze Age) and the Western probably later. The end of working in the Northern group seems to have been due to a change of policy, perhaps linked to the cultural break between the Urnfield and Hallstatt periods, but the working of the Eastern group was terminated by a disastrous landslide.

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PREHISTORIC SALTMINING AT HALLSTATT

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Morton, Fr.	1957	Weitere Neufunde aus Hallstatt. <i>Archaeologia Austriaca</i> , 22: 32–8.
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For description and position of the relevant sites see Schauburger (1960). The proposed systems are not yet fully worked out, and for this reason the preparation of definitive plans is not possible at present. For the Stügerwerk area an effort towards this direction has recently been made (Barth, 1982).

The Exploration of the Long-distance Movement of Bronze in Bronze and Early Iron Age Europe

by J. P. NORTHOVER

For many years the analysis and interpretation of prehistoric bronze metallurgy in Europe have been inhibited by an overriding concern with establishing a direct relationship between the metal produced and the ore sources from which it derives. Additional problems stem from the belief that after the Early Bronze Age the use of scrap became such a dominant feature of metallurgy that the search for such relationships was impossible. In fact the use of scrap can be demonstrated in virtually all periods while, at the same time, it is possible to reach important conclusions about the nature of the ore sources in use. Also, there has always been concern expressed about the relative absence of ingot tin and copper in the archaeological record except in special circumstances. All these difficulties have arisen from the persistence of well-established but untested assumptions and a dearth of constructive thought about the many ways in which metal might have been circulated, deposited and recovered to produce the archaeological record as we see it today. We should always first take the record at face value when there is no overwhelming reason to do otherwise.

The consequence is that there are large areas of Europe where there is a great preponderance of earlier Bronze Age analyses (e.g. Otto and Witter, 1952; SAM, 1-4) and very little from the later Bronze Age and even less from the Iron Age. As a result we have very little idea of even the typical alloys of these periods for much of Europe. Fortunately in the United Kingdom, in France and, to a lesser extent, in Scandinavia and Switzerland the concern with the identification of ore sources has not, in recent years, been allowed to obscure the other benefits to be obtained from the analysis and metallography of bronze. In turn, this is leading, through a more flexible approach to interpretation, to a thorough re-appraisal of the structure of Bronze Age metalworking and this is the starting point of this paper. The general principles governing our approach to bronze analysis are outlined, followed by a brief description of the way in which the scale of organisation of Bronze Age metallurgy changed with time leading up to some estimate of the nature of the industry in Hallstatt times.

Historically, bronze analysis, because of the availability of suitable apparatus and qualified analysts, has been chemically based, usually in some form of optical spectro-

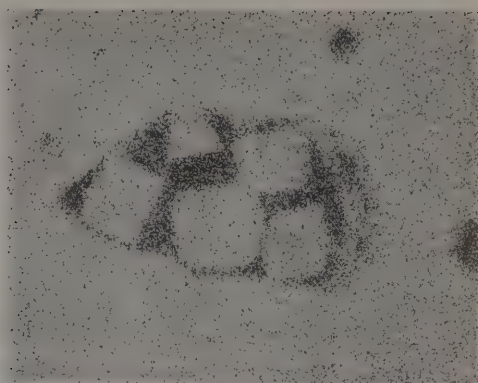
scopy. This approach, although productive and relatively cheap, can easily obscure important information. For example, in much early Irish copper much of the concentration of arsenic and antimony recorded is present as oxide inclusions, sometimes in sufficient quantity to give a spurious indication of a copper-arsenic alloy. At the Department of Metallurgy and Science of Materials at the University of Oxford, and formerly at the University College of North Wales in Bangor, the approach has been firmly rooted in metallurgy and metallography. This has been encouraged by the extensive use of electron probe microanalysis which allows the same solid sample to be used for analysis, inclusion analysis, measurement of segregation, phase identification, grain size measurement, hardness testing, metallography and, if ultimate destruction of part of the sample can be permitted, lead isotope analysis. This immediately gives a much greater range of information for reconstruction of the resources, techniques and organisation of metalworking by identification of common impurity and alloying patterns in unknown groups of material, establishing common technical traditions and determining the state in which artefacts were deposited. The technique also allows the analysis of elements such as oxygen and sulphur, so inclusions which might be fossils of the manufacturing process can be analysed. As modern instruments of this nature are very fully automated it is very simple to process the large numbers of samples required to produce quantitative rather than qualitative descriptions of bronze industries.

In a worst-case analysis, a large scale programme of metal analysis for any, and any should be emphasised, prehistoric bronze industry can be expected to determine the following features of what, after all, was technically the most complex process undertaken in prehistoric societies:

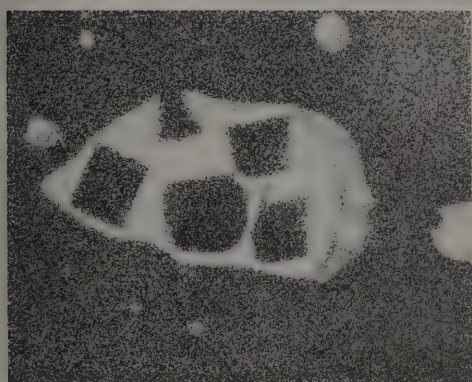
1. Changes in metal refining techniques with time and space; the indicators here are the population of oxide and sulphide inclusions (Plate I) and iron; the appearance of a consistent level of iron can say something about both the ore type and the extraction process.
2. Changes in alloying practice with time and space: the occurrence of major alloy changes within the Bronze Age has been well known for some time (e.g. Brown and Blin-Stoyle, 1959); smaller shifts occur more frequently (Fig. 1, after Northover, 1980), roughly associated with typological change. These changes, which assist in refining bronze-based chronologies, can be the result of local industrial developments or changes in the source area from which primary or secondary bronze suppliers were obtained.
3. Changes in the number and nature of metal sources (not the ultimate location of the primary production centres involved). Generally these changes occur in step with those in alloying and typology but this is not always the case (Fig. 2, after Northover, 1980). For example, the introduction of tin bronze in Ireland on any large scale was associated with a change in axe form but the impurity patterns were unaltered (Northover, in Case, forthcoming). On the other hand, in south-west England the same impurity pattern persisted throughout the



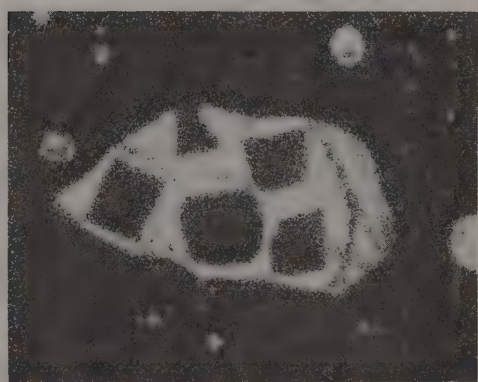
SEI



Cu K_{α}



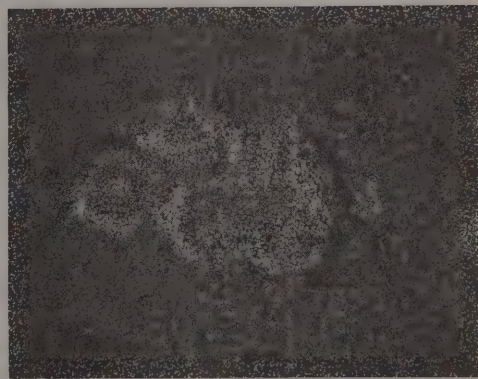
As K_{α}



Sb L_{α_1}



Ag L_{α_1}



S K_{α}

Plate I. Inclusions in arsenical copper (all $\times 1000$).

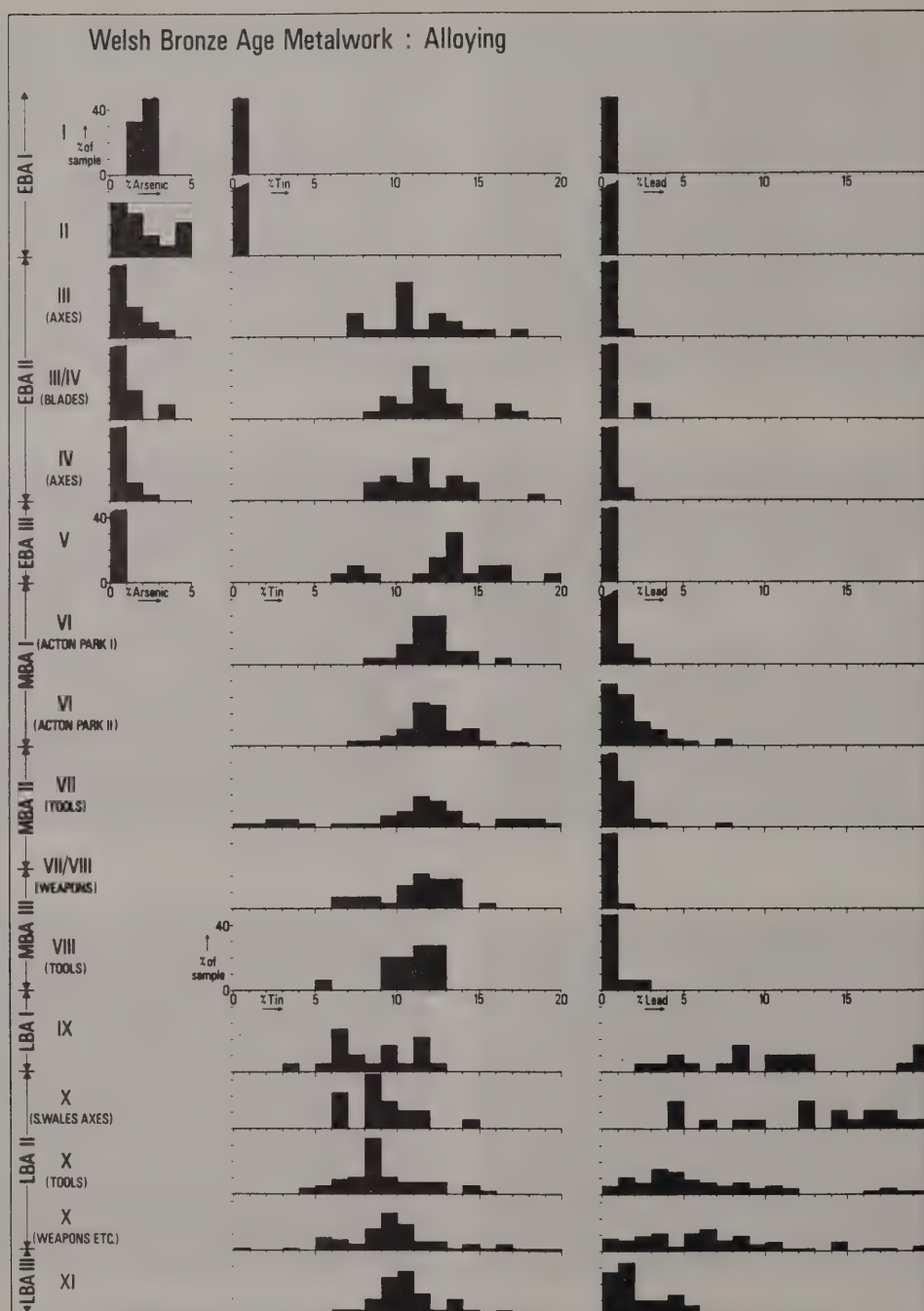


Fig. 1 Variation of alloying with time in Welsh Bronze Age metalwork (after Northover, 1980)

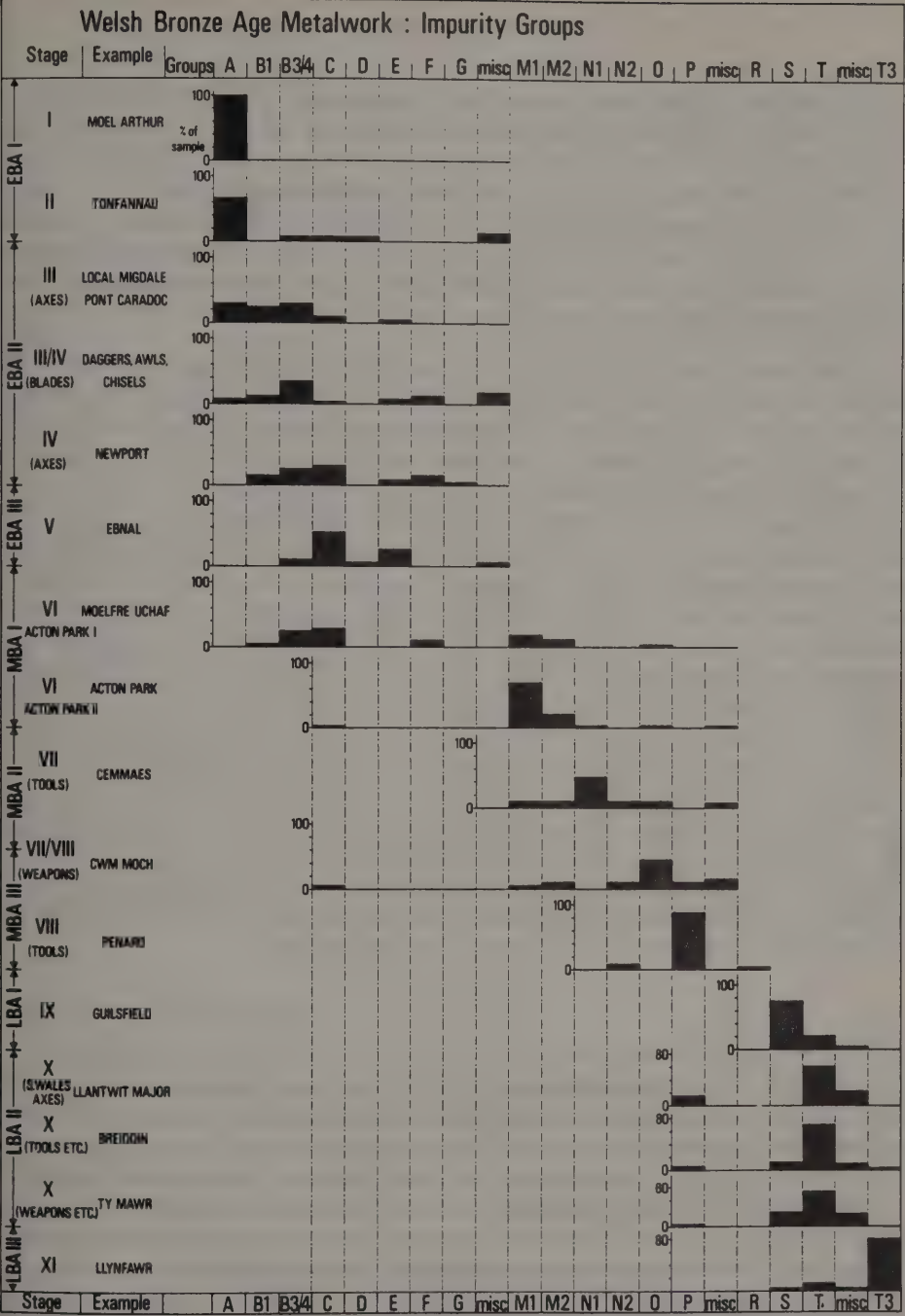


Fig. 2 Variation in occurrence of impurity patterns with time in Welsh Bronze Age metalwork (after Northover, 1980).

Bronze Age and into the Iron Age (Northover and Pearce, forthcoming). Lead isotope and inclusion analyses can provide additional data for identifying metal changes.

4. The use of solid samples means that changes in metalworking practice can be identified metallographically as well as from the external condition of the artefacts. For example, it is frequently possible to determine from the segregation of alloying elements whether an object was cast in a permanent (metal or stone) or clay mould (Northover and Staniaszek, 1982).

The general availability of data of this quality for the whole of the European Bronze and pre-Roman Iron Ages would represent a colossal advance in our knowledge of the economics and technology of prehistoric metalworking. The maximum benefit, however, will only be obtained if typological and metallurgical studies are pursued in close conjunction as each can illuminate puzzling aspects of the other.

We can often take our conclusions further than this. Experience with interpreting the distribution of bronze types in Britain and France has shown that the number of metal sources, either primary or secondary (scrap) used in any area at a given time was very limited, often only one. Given the fact that the only elements severely affected by re-cycling are lead and, when already in the form of oxide, arsenic and antimony, most compositions are unaltered by recycling. With limited opportunity for mixing it becomes possible to follow the movement of metal on a very large geographical scale. Of course, within the overall circulation area of a metal type there may be a variety of organisational patterns depending on, among other things, the proximity of the primary source and the social structure supporting the secondary industry. These ideas form the basis of our reconstruction of the metal trade. A brief outline of events in the earlier parts of the Bronze Age is followed by a more detailed account of times closer to the subject of this meeting. The descriptions centre on the British Isles, where the necessary data are best organised, and work outwards from there. They are solely concerned with the circulation of metal and not with the reconstruction of exchange systems.

Structures

Throughout the Bronze Age, and, just possibly, in the Iron Age as well, there were generally three distinct zones of bronze used in the British Isles: Ireland, the Highland Zone and the Lowland Zone. The first two have indigenous non-ferrous metal resources and were usually able to support some autonomous primary production of bronze. The evidence suggests that alloying with tin was carried out at or near the primary production centres; alloying with lead could take place there, or where secondary bronze supplies are involved, near the lead source or at a secondary production centre (Northover, 1982; Lawson 1979). The Lowland Zone always had to rely on the direct import of objects for use or on secondary resources, almost always scrap from the European mainland. Present evidence suggests that imports of scrap predominated.

Each of these zones had its own sub-divisions. From the correlation of typology and metallurgy has emerged the concept of a *metal circulation zone* in which metal of common impurity pattern (or patterns) and alloy type (or types) is used. The qualifications are necessary as in some periods two or three metal types were used with equal freedom in the same circulation zone. Within the *metal circulation zones* local industries exist which can be identified typologically and various patterns of the use of primary and secondary metal determined, e.g. by the existence of a proportion of alien types or by the presence or absence of founders' hoards of scrap. Other differences might appear between the systems for the production of everyday equipment and for prestige and fine metal-work. As the Bronze Age progressed these differences increasingly reflected a technological divergence between the two classes of production related to the very different inputs of labour, energy and skill required. As the production of prestige metal-work became more specialised it could become more centralised and a rather uniform product, say a Ewart Park sword, could achieve a far wider distribution than any localised tool type although local imitations and variants of prestige products could still be made.

The boundaries between these *metal circulation zones* can be quite sharply defined and the bulk movement of metal across them severely limited. More mobile types, e.g. the more elaborate weapons and ornaments, could have been more freely exchanged. It is also less likely that such prestige items would be reduced to scrap except through failure or obsolescence; other means existed for the withdrawal of this material from circulation at the end of its useful life, e.g. the deposition of swords in rivers. These events influence both the distribution of metal in circulation in prehistoric times and the archaeological record as we see it today.

These concepts also have some validity on the Continent, at least in Atlantic Europe. The history of Bronze Age metallurgy in all these areas should now be very much concerned with charting the rise and decline of individual elements in this structure, their association with technological change and the resulting movements of zone and local area boundaries.

Earlier Bronze Age metallurgy

At the beginning of metal use the three-zone structure had not yet evolved in the British Isles. The first substantial use of metal was in Ireland, most probably based on the ore resources of the south-west. This industry seems to have supplied the bulk of the metal used in the copper-using period in Britain as well as Ireland. At the same time there was a gradually increasing import of metal from the Continent, generally of or for daggers and halberds; these show some evidence of Cu-As alloying, a practice never regularly adopted in the British Isles. Towards the end of this period there are the first beginnings of indigenous copper production in the Highland Zone. This pattern was continued into the earliest bronze-using period with the additional use of European metal types for tools. As the use of tin bronze appears to have developed more rapidly in the British Isles than in adjacent parts of Europe the possibility exists that tin additions were made in Britain

Copper – using period

Secondary production & distribution areas ★

Use of local ores L

Local re-cycling in most areas

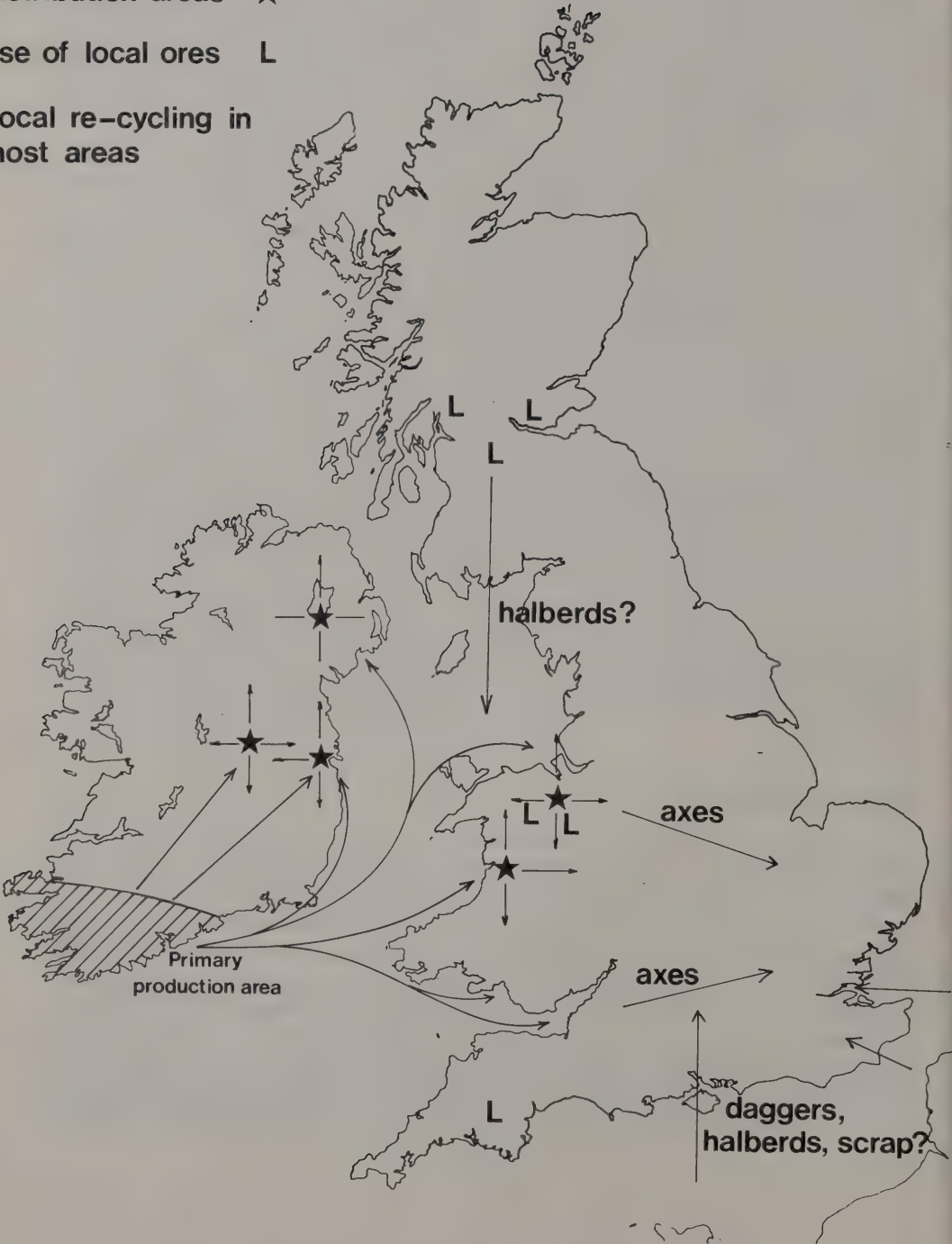


Fig. 3 Organisation of metal-working in the British copper-using period.

VELOPED EARLY BRONZE AGE



Fig. 4 Regionalisation of metal-working in the developed Early Bronze Age in the British Isles.

to copper of European origin. A similar case with lead alloying will be seen in the later Bronze Age. The structure of the earliest metal-working is shown in Fig. 3.

The first major break in this pattern occurred when the first Irish industry collapsed through causes as yet unknown. This gave an opportunity for the development of local extraction and production in the Highland Zone and for new industries to grow up in Ireland. Nevertheless there are few areas outside Ireland as yet where we can speak of a full Bronze Age, that is a period where bronze products have substantially replaced other products for the manufacture of everyday equipment such as axes; it is perhaps better to continue to refer to a bronze-using period. The contrast between Ireland and Britain is also seen in the organisation of production. In the Highland Zone of Britain production remained localised with several production centres in Wales alone while in Ireland three industries traded throughout Ireland and continued to maintain an export trade to Britain and, probably, the Continent. Meanwhile the use of European metal continued to expand in Lowland Britain; more than one type can be identified but the relationship between them appears to be chronological rather than geographical. This pattern, with variations in the success of individual areas, lasted until the end of the Early Bronze Age and into the beginning of the Middle Bronze Age (Fig. 4).

The next major change is metallurgical rather than strictly typological. Roughly contemporary with the development of the first phase of the Middle Bronze Age in Wales metalworking there switched from the existing regional structure for *metal* production to a single, nationally distributed bronze type, associated with the first introduction of lead alloying, but still displaying a regionalised axe typology. This event coincided with a recession in the cross-Channel movement of bronze so that the Welsh industry developed to supply much of Lowland Britain with metal and also exported to Brittany, the Netherlands and, probably, the intervening Channel coasts (Butler, 1963; Northover and Burgess, forthcoming). This is the only period in the Bronze Age when such success can be attributed to a British (rather than Irish) primary copper producer (Fig. 5). This success reached a peak and then a slow decline set in throughout the rest of the Middle Bronze Age (Figs. 6, 7). As the frontier of the north-west north French Lowland British circulation zone moved north-west in response to this reduction in pressure so encroachments were also made from Ireland and the only outlet was probably to south-west England. The decline is also seen in a shift in the production of the few prestige objects seen in Wales at this time — rapiers: in the time of expansion they were made of Welsh metal but as decline became steeper they were replaced with Irish production while the Welsh producers reverted to tools and a few very simple spearheads.

We should now, at last, begin to look firmly towards Europe. The north-west/north French/Lowland British metal in the middle years of the Middle Bronze Age was a very distinctive high tin bronze with a single impurity pattern dominant (Northover and Pearce, forthcoming). It is not yet clear whether this distribution is at the end of an Atlantic or Alpine supply chain, nor are the reasons behind the consistent use of 13–17% tin alloys clear (Fig. 6). At the end of this period, the beginning of the Penard period in Britain (Burgess, 1976) (i.e. Bronze Final I in France and spanning Reinecke D and Ha

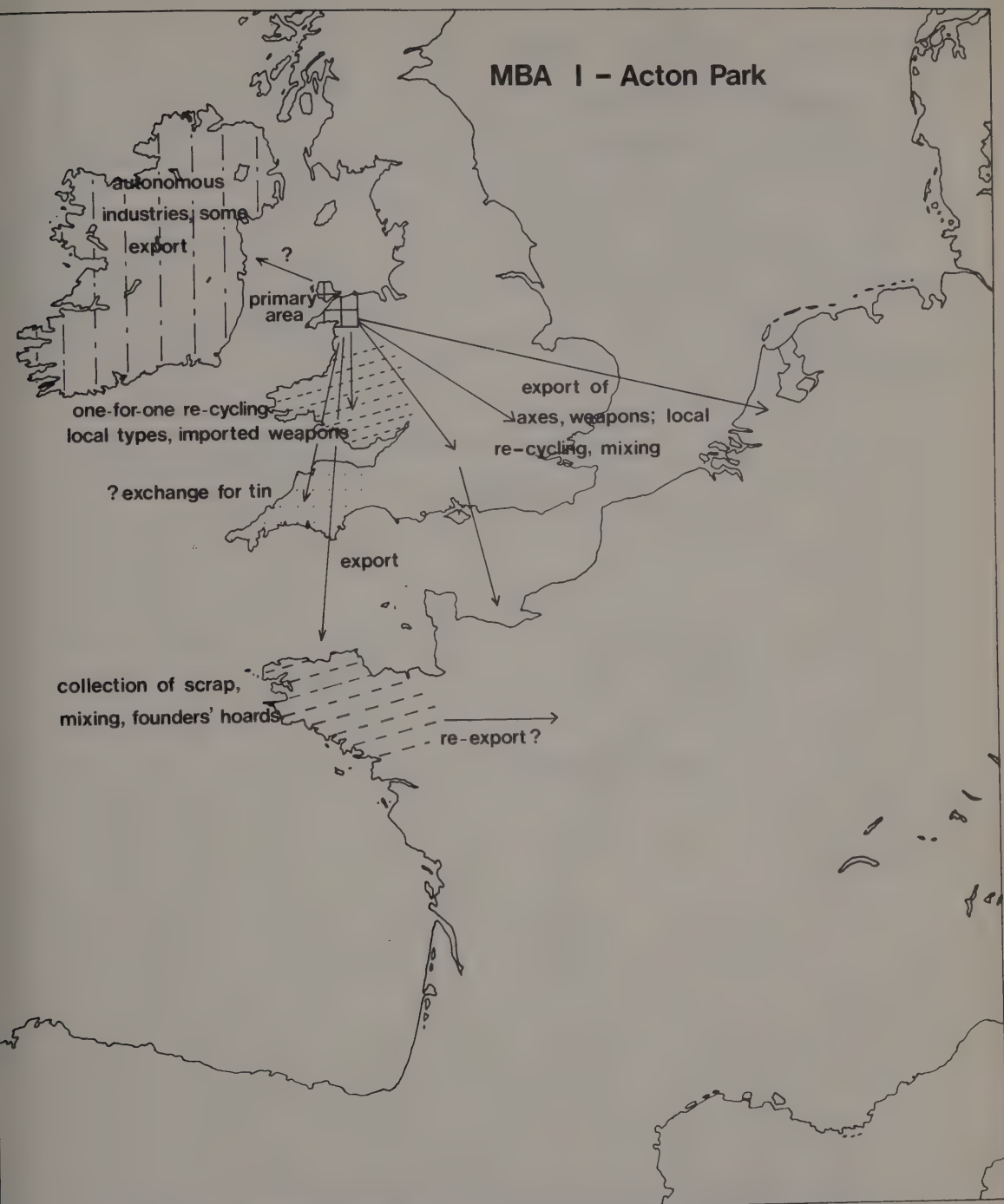


Fig. 5 Organisation of metal-working in the Acton Park phase (MBA I).

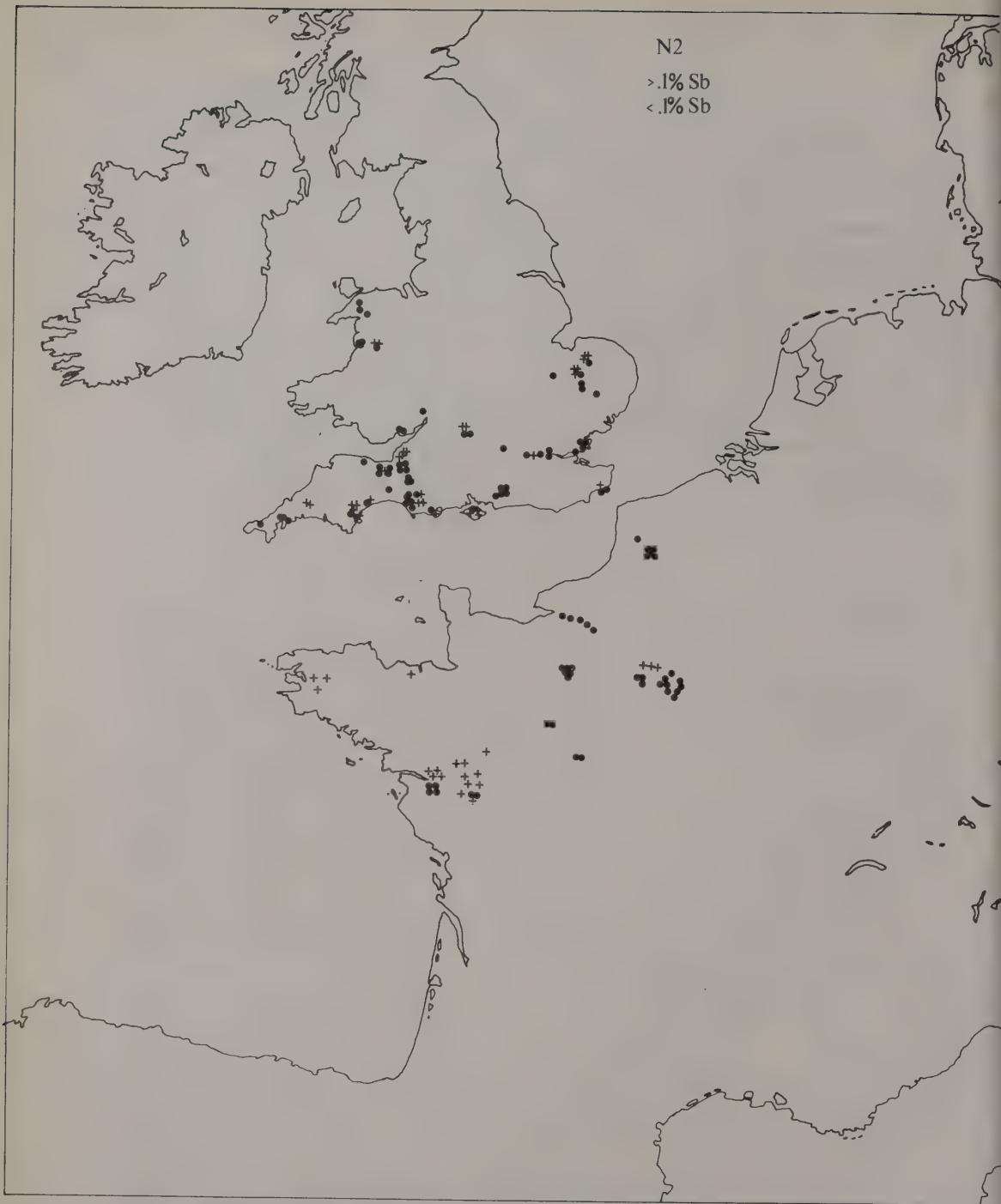


Fig. 6 Distribution of north-west/north French/Lowland British composition group in the Taunton Phase (MBA II).

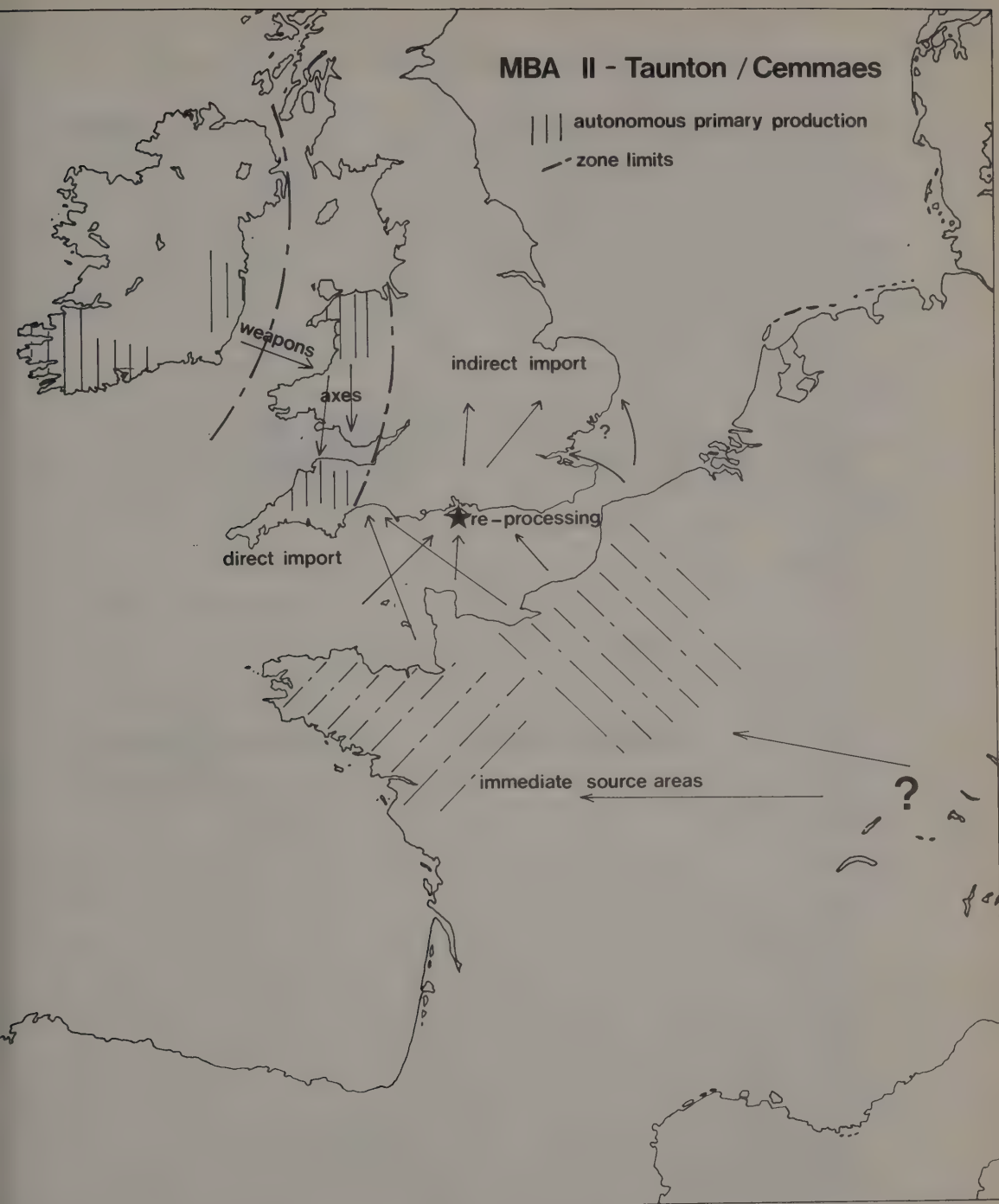


Fig. 7 Organisation of metalworking in the Taunton and Cemmaes industries (MBA II).

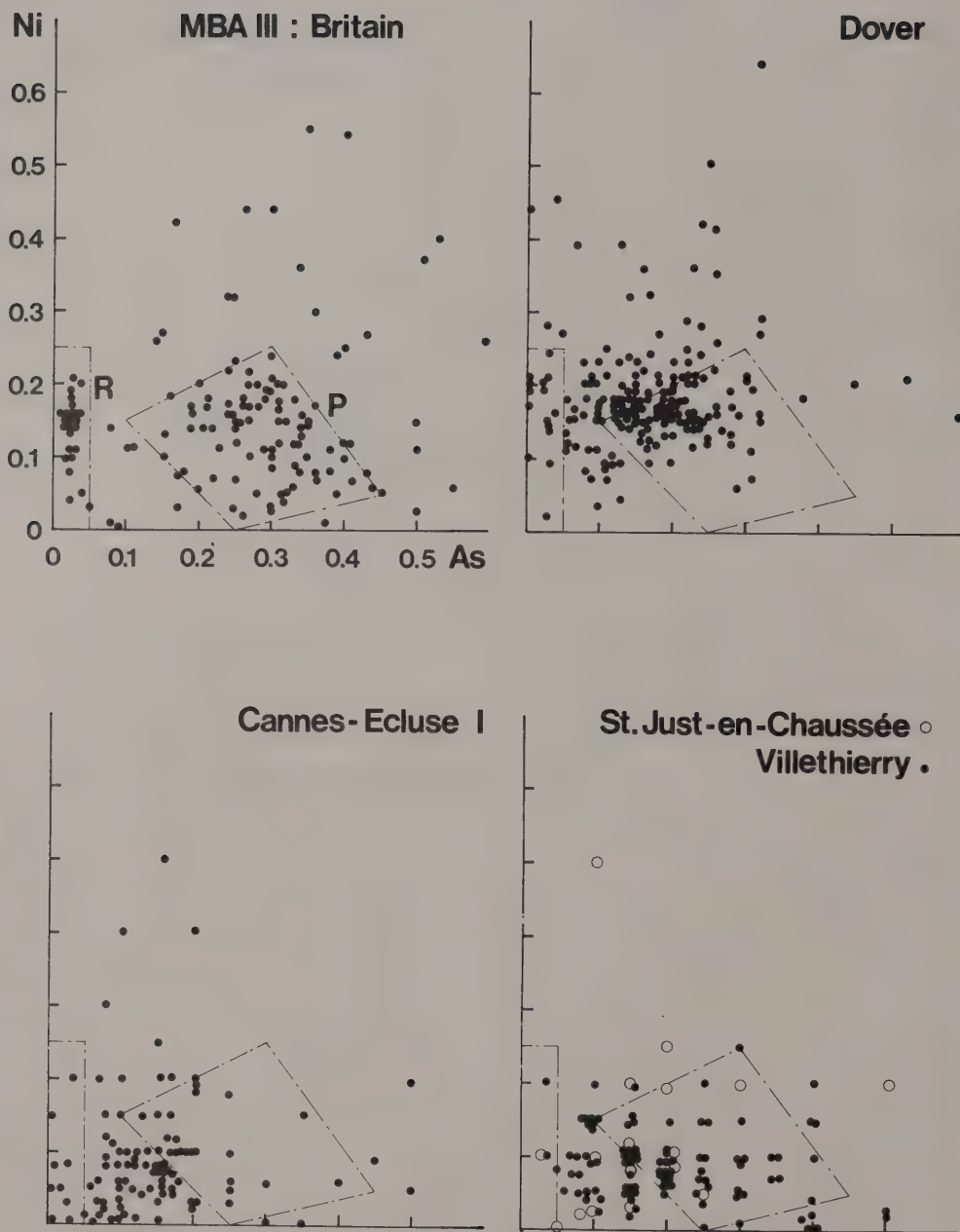


Fig. 8 Bronze compositions in Middle Bronze Age III/Bronze Final I.

A1/A2 further east) major shifts in the pattern of bronze supplies again took place. The Penard period in the British Isles is dominated by a single alloy type (8–11% tin) and a pair of related impurity patterns (labelled for convenience 'P' and 'R', Northover, 1980); metal of this type is also very widespread in France and is also seen in northern Germany. The overall pattern in Britain probably represents the average of a number of small variations as indicated by the Dover 'shipwreck' (Muckleroy, 1981), a presumed import cargo of bronze, and a number of roughly contemporary French hoards (Fig. 8). We cannot as yet put an overall direction to the movement of this metal; the distribution of 'P' metal in Britain is shown in Fig. 9. None of these periodic transitions was abrupt. In Britain the Dover group contains about 10% material with analyses more typical of the preceding Taunton phase, while the Taunton, Somerset, hoard (Smith, 1959) has a small element, notably among the ornaments, with Penard compositions. The corresponding changes in France have not yet been worked out in detail so it is not clear whether the shift is due to a change in immediate source area in France or is part of some larger-scale process.

Later Bronze Age metallurgy

Towards the end of the Penard period, contemporary with Ha A2, a new and distinctive metal type becomes increasingly common. This has As, Sb, Co, Ni and Ag as its major impurities in contrast to the As/Ni patterns of preceding phases. The last appearance of such metal on any scale in Britain was in the Arretton industry at the end of the Early Bronze Age (Britton, 1961). The earliest re-appearance of this impurity pattern, labelled 'S', is associated with the development of the flange-hilted sword in Britain and virtually all the examples so far located are from the Thames Valley. The use of this type of metal is then greatly expanded as it is effectively the sole bronze resource for the Wilburton industry at the start of the Late Bronze Age in the southern half of Britain (contemporary with Bronze Final II in France and Ha B1 further east).

This industry has been the subject of an extensive research programme at Oxford which has served to emphasise the unusual nature of the industry in the British context (Northover, 1982). The origins of 'S' metal appear to be at least Alpine, more probably farther east than that in Central Europe. It is similar to metal common in those areas in earlier times and we can plot a widespread distribution for it which points in that direction. The distribution has a well-defined south-western boundary at the lower Loire which confirms that it is not an Atlantic one. The same bronze type occurs in Bronze Final II industries at St Brieuc-des-Iffs in Brittany and in the Seine basin but not in similar products from St Denis-de-Pile much further south-west (Chevaillot, 1981). The 'S' composition is seen in a variety of recently analysed Ha B2/B3 products from Switzerland (Rychner, 1981) and from the Rhineland. It is accompanied by a declining tin content which seems to have reached a minimum in Ha B2 when there may have been an attempt to substitute antimony for tin. The effects of this are seen in finds from as far



Fig. 9 Distribution of the 'P' composition group in the Penard phase (MBA III).

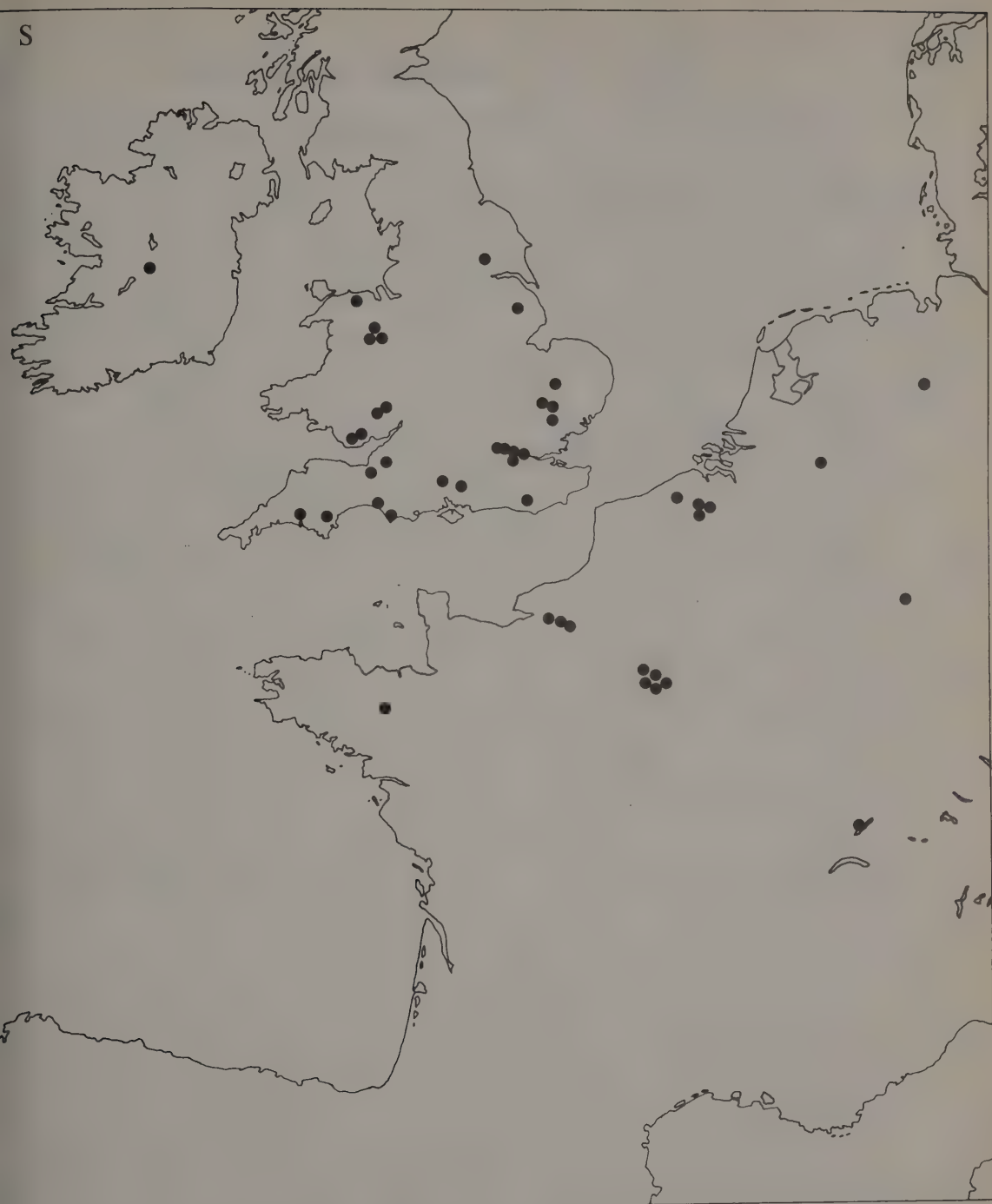


Fig. 10 Distribution of the 'S' composition group.

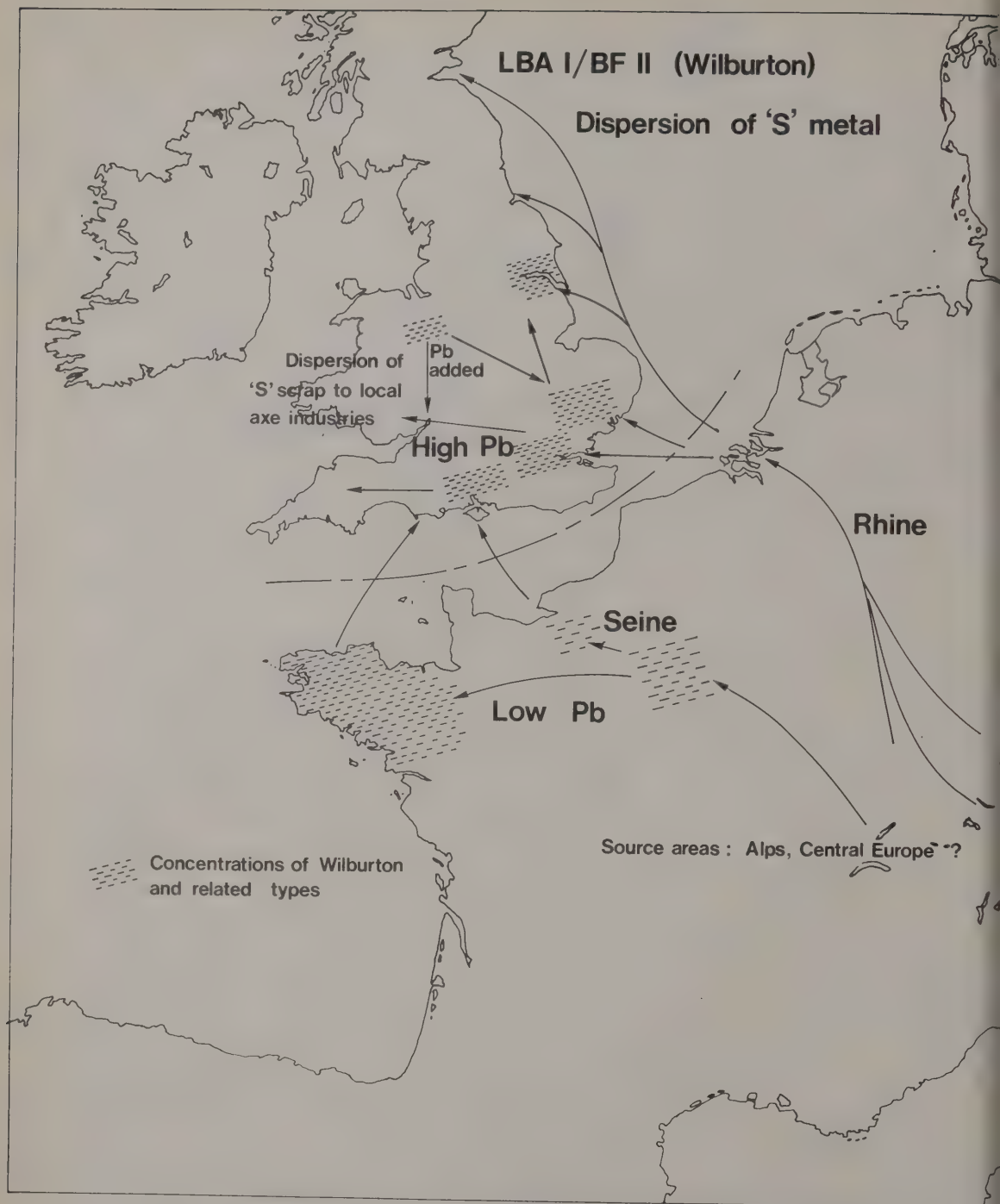


Fig. 11 Organisation of Wilburton metalworking and the dispersion of 'S' metal (LBA I/BF II).

apart as Hallstatt (Table 2) and East Anglia (Craddock, 1980). The distribution of 'S' metal is shown in Figs. 10, 11.

In Britain the Wilburton industry also made extensive use of lead as an alloying element and had a highly developed casting technology making a variety of complex shapes and thin-walled hollow sections. The full development of both occurred in Britain. The use of high-lead alloys was entirely a British development in response to local technical and economic demands although low-lead alloys were in use in the French industries of Bronze Final II (Table 1) and formed the bulk of imported scrap. The inspiration of some of the more elaborate products was drawn from a wider area: lozenge-sectioned chapes from France, hollow-bladed spearheads perhaps from northern Europe (Jacob-Friesen, 1967).

In Britain lead contents climbed to over 20% but the impact on the properties of the bronze is not as severe as previously believed (Northover and Staniaszek, 1982). Lead isotope analysis suggests the use of a single lead source in Britain (Northover and Gale, 1982). The use of small amounts of lead for alloying to improve casting (and in fact mechanical) properties had disappeared from Britain during the Middle Bronze Age. The practice appears, as suggested above, on a small scale in some metalwork at the end of Ha A, apart from exceptional items such as 40% lead in an arrowhead from the Penard hoard itself (Northover and Burgess, forthcoming). The use of large quantities of lead may have been intended simply to lower the liquidus temperature and viscosity of melts to assist the filling of complex moulds but it would also have been a significant factor in extending the metal supply in an industry otherwise entirely dependent on imported scrap. It is possible to interpret the industry as being alien and elitist, imposed on the existing pattern of evolving local tool industries and cut off by native hostility from British copper and tin. Surrounding the limited Wilburton region the older pattern of metal supply persisted, still with Penard type compositions, but clearly influenced by some Wilburton models and eventually obtaining some Wilburton metal. This rather isolated position may partly explain the short life of the Wilburton industry and the elite that supported it. This is perhaps reflected in the rapid decline in the demand for its more elaborate castings in the immediately succeeding period (Figs. 11, 12).

In the Atlantic parts of Europe 'S' metal is so distinctive that it is easy to plot the history of its use. As the Wilburton industry progressed 'S' scrap made its way to local tool industries and the technique of excessive lead use was adopted by some (Northover, 1980) while in others the presence of lead was probably residual. The Wilburton industry put so much lead into circulation that its effect must have lasted a long time and has distorted our views of lead use. Events at the end of the Wilburton period could have been dramatic. The bulk of late Wilburton material is concentrated in a small number of scrap hoards which are a fossil of the industrial practice and metal distribution system of that time. The hoard evidence shows a progressive decline in the import of 'S' metal, first on the south coast and rather later on the east coast. On a broader scale this is being caused by an expansion of the Carpi's Tongue province northward into northern France and south-east Britain. This cut the southern transport route for 'S' metal via the Jura, the

Table 1 Analysis of metalwork from the Musée des Antiquités Nationales

Sample	Cat.	Provenance	Type	Sn	As	Sb	Pb	Co	Ni	Fe	Ag	Au	Zn
MAN 1	73904	Saint-Brieuc-des-Iffs (73)	Chape	8.28	0.64	0.59	2.7	0.08	0.53	tr	0.14	0.10	—
MAN 2	73904	Saint-Brieuc-des-Iffs (69)	Chape fragment	10.75	0.82	0.76	4.4	0.08	0.28	0.02	0.25	tr	tr
MAN 3	73905	Saint-Brieuc-des-Iffs	Spear fragment	8.98	1.25	1.44	3.1	0.07	0.31	0.03	0.42	—	—
MAN 4	73910	Saint-Brieuc-des-Iffs (134)	Pin or rivet	8.75	0.85	1.01	3.3	0.04	0.20	0.02	0.34	—	tr
MAN 5		Saint-Brieuc-des-Iffs (107)	Rod	4.94	0.32	0.26	0.05	0.06	0.26	0.05	0.11	—	—
MAN 6		Saint-Brieuc-des-Iffs (119)	Pin fragment	10.81	0.06	—	2.5	0.15	0.27	0.10	tr	0.05	0.04
MAN 7		Saint-Brieuc-des-Iffs (75)	Gouge	7.19	0.86	0.98	6.1	0.01	0.21	0.03	0.26	—	—
MAN 8		Saint-Brieuc-des-Iffs (91)	Pin fragment	0.27	0.60	0.73	0.13	tr	0.18	0.02	0.60	—	—
MAN 9	73902	Saint-Brieuc-des-Iffs (76)	Gouge	6.91	0.56	0.60	0.79	0.01	0.28	0.01	0.20	—	tr
MAN 10		Saint-Brieuc-des-Iffs (B)	Sheet	9.45	0.98	1.08	4.3	0.04	0.28	0.02	0.33	tr	tr
MAN 11		Saint-Brieuc-des-Iffs (71)	Chape fragment	11.03	0.90	0.75	2.1	0.06	0.21	0.02	0.24	tr	0.04
MAN 12		Saint-Brieuc-des-Iffs (A)	Sheet	10.09	0.92	1.25	1.9	0.06	0.35	0.03	0.39	0.04	—
MAN 13		Saint-Brieuc-des-Iffs (111)	Rod	22.55	0.67	0.85	4.4	0.01	0.16	0.02	0.27	tr	tr
MAN 14		Saint-Brieuc-des-Iffs (105)	Spiral fragment	4.92	0.24	0.23	0.20	0.07	0.26	0.06	0.13	—	tr
MAN 15	26019	Essonne	Spearhead	7.52	0.25	tr	0.08	0.06	0.11	—	0.06	—	0.03
MAN 16	71818	R. Seine, Essonne	Spearhead	10.38	0.83	5.75	8.3	0.02	0.84	—	0.43	—	—
MAN 17	26325	R. Seine, Essonne	Spearhead	4.80	0.62	0.92	tr	0.04	0.22	tr	0.34	tr	—
MAN 18	71818	R. Seine, Essonne	Spear socket	9.97	0.15	0.07	2.1	0.02	0.07	0.02	0.05	0.06	—
MAN 19	10478	R. Seine, Paris	V-hilt sword	7.24	0.10	—	4.0	0.01	0.04	0.01	0.08	tr	—
MAN 20	71818	R. Seine, Essonne	Spear blade	6.27	0.92	1.00	0.20	0.05	0.22	0.07	0.33	—	0.07
MAN 21	96334	R. Seine,	Sword	7.14	0.66	0.85	0.30	0.04	0.24	0.05	0.34	—	—
MAN 22	56315	R. Seine, Paris	Sword	6.16	0.45	0.31	0.20	0.39	0.10	0.08	0.11	0.05	—
MAN 23	26032	Paris	Sword	5.24	1.31	1.41	0.80	0.04	0.20	0.09	0.56	0.04	—
MAN 24	56315	R. Seine, Paris	Sword, dec.	3.32	1.11	1.20	5.3	0.03	0.20	0.01	0.38	tr	—
MAN 25		R. Seine	Bronze mass — button	6.24	1.04	0.99	0.30	0.01	0.15	0.04	0.37	—	tr
MAN 26		R. Seine	Bronze mass — sword hilt	4.72	1.49	1.54	0.50	0.05	0.25	—	0.44	tr	—
MAN 27	2051	Champeuil	Sword hilt	7.10	1.12	0.87	1.7	0.10	0.41	0.02	0.28	tr	tr
MAN 28	2052	Champeuil	Sword fragment	3.88	0.51	0.70	0.70	0.02	0.24	0.08	0.24	—	0.03
MAN 29	2054	Champeuil	Axe fragment	7.58	1.06	1.33	2.1	0.08	0.31	0.03	0.36	tr	—
MAN 30		Paris	Chape	12.79	0.54	1.45	1.1	0.02	0.26	0.02	0.50	—	tr
MAN 31		Paris	V-hilt sword	6.28	0.28	0.06	—	0.06	0.22	tr	0.07	—	—

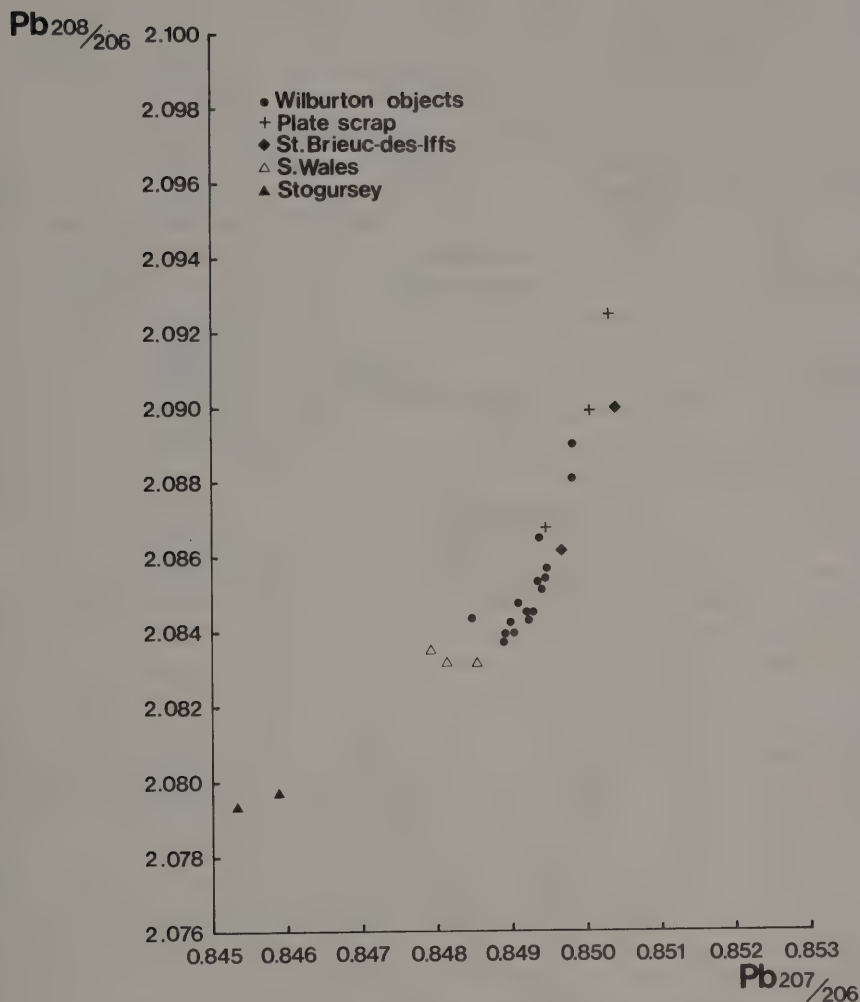


Fig. 12 Lead isotope ratios in British Late Bronze Age metalwork.

Seine Basin and the south coast completely and increasingly affected the imports of 'S' metal via the Rhineland into eastern England. The metal typical of Carp's Tongue hoards is tending to revert to an AS/Ni impurity pattern, although usually modified by residual 'S' scrap. The practice of large lead additions had by this time spread to Carp's Tongue metalworking but this was very erratic and no pattern has yet emerged. The

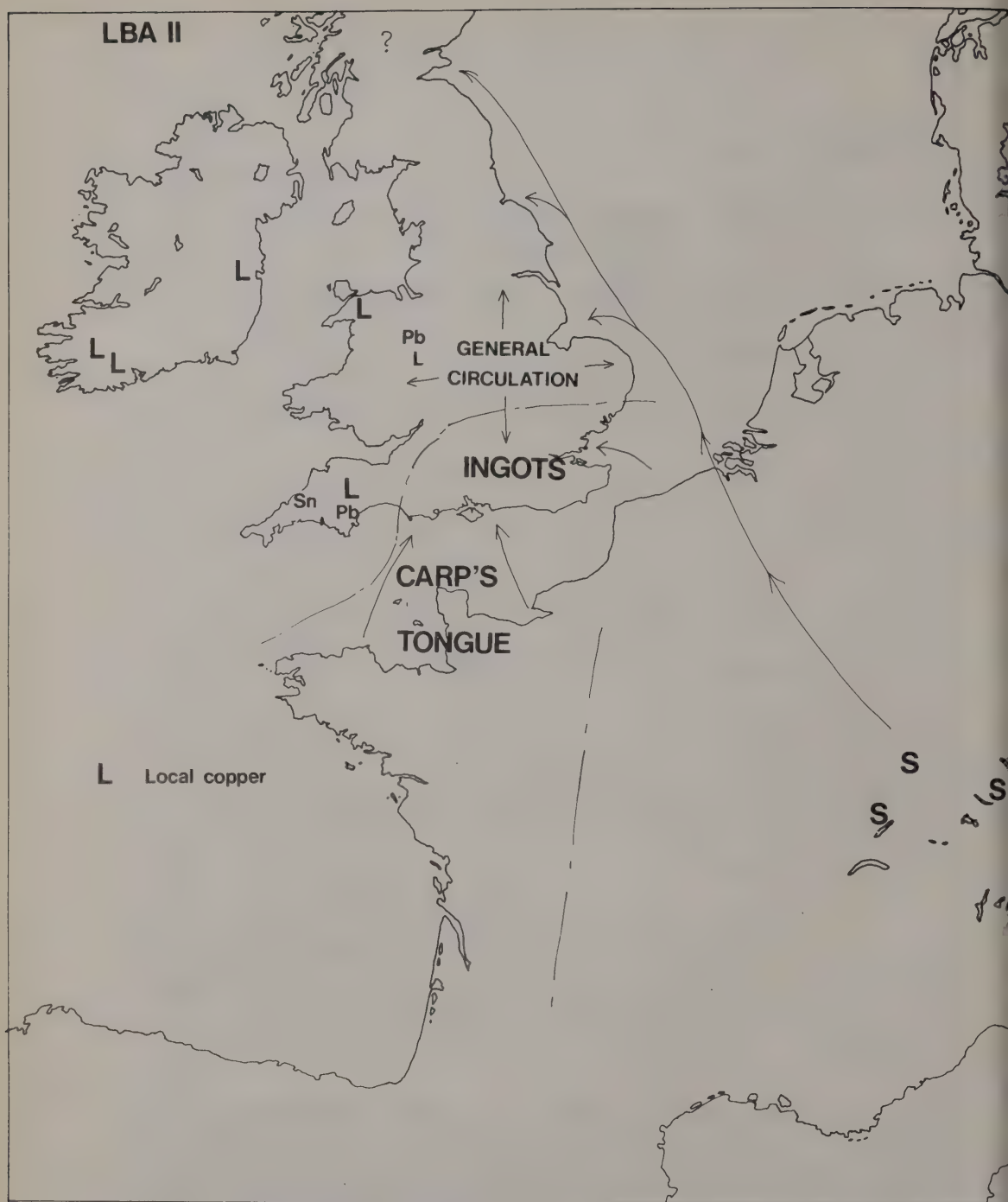


Fig. 13 Organisation of metal-working in the Ewart Park period.

copper ingots in Carp's Tongue hoards are clearly a Carp's Tongue phenomenon and do not relate to British sources of copper. The appearance of so much crude copper in the archaeological record could indicate a problem with the supply of tin like that already suggested for Ha B2. At this time (Ha B2/B3) 'S' metal is still strongly evident in the Alpine area, as at Auvernier (Rychner, 1981), and was still moving down the Rhine to eastern England where it persists well into the Ewart Park phase (contemporary with Bronze Final III, Ha B2/3) (Figs. 11, 13). From the analyses available at present it seems that 'S' metal is more strongly associated with swords and other weapons (Northover, forthcoming) and fine metalwork such as the hoard of horse trappings from Parc-y-meirch in northern Wales (Savory, 1976) rather than tools and other everyday equipment, evidence of a continuing social differentiation in metal-working. At the same time in Britain there is a gradual expansion in the use of indigenous metal resources but this still tends to be confined close to Ireland and the Highland Zone. A difficulty in interpreting the British results is the lack of a comprehensive corpus of Late Bronze Age tools for southern England which prevents the development of any refined internal chronology for the long period of time commonly assigned to this phase. However, the typology of the tools is sufficiently well developed to show up a number of regional types and there are definite regional variations in composition and alloying practice but many hundred more analyses are required to elucidate this completely (Fig. 13).

In the Ha C period the number of available analyses is much decreased but three important British groups, from south-east Wales (Llynfawr and Cardiff) (Northover and Burgess, forthcoming), Mountbatten, Devonshire (Northover and Pearce, forthcoming), and Danebury, Hampshire (Northover in Cunliffe, 1982), have been studied. These analyses serve to show a noticeable improvement in the quality of the bronze in use, an increase in the size of axe castings and a continuing reduction in the use of lead. There is still some regional variation in impurity patterns: the metal at Llynfawr appears to contain a large element that is residual from the preceding period in that area, that at Danebury more like that from northern France while that at Mountbatten is a mixture of the two and is also influenced by local production and Armorican socketed axes. Indeed there is a bronze cake from Mountbatten that appears to derive from a re-melted Armorican socketed axe. Sporadic finds of 'S' metal still appear in the west: an axe at Llynfawr and a razor from Bernissart in Belgium (Table 2). However, a number of Hallstatt C objects from Hallstatt itself has recently been analysed at Oxford (Table 2) and they too show a marked decline in the incidence of 'S' metal, a decline in the use of lead and an increase in the availability of tin. Indeed, the results from Hallstatt and from southern Britain look remarkably similar. Is this because they are the outcome of the same processes or because metal was still being moved over these distances and we are seeing a re-orientation from an Atlantic to an east-west distribution such as that associated with the emergence of the Wilburton industry? Indeed, it could be argued that the metalwork of the Wilburton and Hallstatt C groups in Britain share many social characteristics and could be products of the same type of process. Both views are plausible and may be part of the final picture. A lot of analysis needs to be done to fill the gaps.

Table 2 Analyses of European material in the Ashmolean Museum, Oxford

Sample	Provenance	Museum No.	Type	Sn	As	Sb	Pb	Co	Ni	Fe	Aq	Au	Zn
Ash 11	R. Seine, Paris	1927.2264b	Ferrule	9.28	0.47	0.70	0.3	0.04	0.14	0.02	0.27	tr	—
Ash 12	R. Seine, Paris	1927.2243b	Ferrule	4.82	1.07	1.44	4.2	0.03	0.18	0.02	0.34	tr	—
Ash 13	R. Seine, Paris	1927.2260a	Spearhead	6.38	0.54	0.89	0.1	0.04	0.16	0.02	0.29	0.08	0.05
Ash 14	R. Seine, Paris	1927.2261e	Sword hilt	6.88	0.60	1.00	0.5	0.02	0.15	—	0.36	tr	—
Ash 15	R. Seine, Paris	1927.2262	Sword blade	5.09	0.98	1.63	0.5	tr	0.22	tr	0.51	—	—
Ash 16	R. Seine, Paris	1927.2666b	Casting waste	7.01	1.51	1.44	1.3	0.02	0.16	0.02	0.46	—	0.04
Ash 17	R. Seine, Paris	1927.2262	Spearhead	4.29	0.94	0.86	0.4	0.02	0.21	0.03	0.33	0.04	—
Ash 18	Chalons/Saône	1927.2043	Sickle	9.63	0.20	0.29	0.9	0.04	0.17	tr	0.15	0.07	0.03
Ash 19	Dreuil, Somme	1927.2312	Sheet	13.18	0.25	0.21	2.9	0.02	0.08	0.01	0.08	—	—
Ash 20	Dreuil, Somme	1927.2285	Slide	16.52	0.03	0.10	5.2	0.01	0.10	0.01	0.03	tr	—
Ash 21	Dreuil, Somme	1927.2284	Slide	7.67	0.13	0.07	1.3	0.02	0.09	tr	0.02	0.04	—
Ash 22	Dreuil, Somme	1927.2275	Sword	8.48	0.07	0.01	1.9	0.02	0.06	—	0.05	0.10	tr
Ash 23	Dreuil, Somme	1927.2267	Socketed axe	14.15	0.29	0.05	4.1	0.01	0.04	0.01	0.06	0.04	—
Ash 24	Dreuil, Somme	1927.2268	Socketed axe	12.07	0.28	tr	11.0	0.03	0.06	tr	0.03	—	—
Ash 9	Rhineland	1927.1660	Sword	14.51	0.17	0.07	2.5	0.02	0.04	—	0.03	0.03	—
Ash 10	R. Rhine, Mainz	1968.670	Sword	9.23	0.47	0.83	0.2	0.16	0.55	0.07	0.26	0.06	—
Ash 41	Bonn	1927.1590	End-winged axe	7.62	0.44	0.65	1.0	0.05	0.19	0.04	0.18	—	—
Ash 6	Hallstatt	1927.1526	Socketed axe	0.34	1.06	3.65	0.6	0.02	0.04	—	0.03	0.03	—
Ash 34	Hechtsheim	1927.1612	Arm-ring	6.35	0.20	0.06	1.7	0.03	0.16	0.04	0.08	—	0.11
Ash 36	Hechtsheim	1927.1613	Arm-ring	8.15	0.21	0.05	0.9	0.02	0.48	0.02	0.11	0.06	—
Ash 37	L. Geneva	1927.1980	Sword	7.66	0.23	0.05	0.2	0.03	0.10	—	0.03	—	A
Ash 33	Auvernier	1927.1044	Knife	7.79	0.67	0.45	0.2	0.09	0.62	tr	0.09	0.06	0.20 B
Ash 31	Auvernier	1927.2093	Median-winged axe	8.16	1.05	0.96	0.2	0.49	1.40	0.29	0.19	0.03	0.05 A
Ash 30	Albert, Somme	1927.2239	Short sword	10.31	0.44	0.62	1.1	—	0.12	0.35	tr	—	4.01 B

THE EXPLORATION OF THE LONG-DISTANCE MOVEMENT OF BRONZE

Ash 4	Hallstatt	1927.	Belt pendant	11.92	0.21	0.05	0.1	—	0.10	0.02	0.08	—	0.05 C
Ash 38	Hallstatt	1927.911	Bracelet	5.36	0.28	0.19	0.2	0.08	0.72	0.01	0.12	0.04	— C
Ash 39	Hallstatt	1927.925	Bracelet	6.21	0.29	0.41	0.3	—	0.25	0.02	0.23	0.04	0.02 C
Ash 7	Hallstatt	1927.1007	Sword hilt	10.27	0.26	0.42	0.3	0.57	0.06	0.46	0.09	0.08	B3
Ash 3	Hallstatt	1927.897	Boss	11.37	0.39	0.35	0.6	0.01	0.08	0.06	0.09	—	C
Ash 40	Hallstatt	1927.1009	Sword pommel	7.48	0.55	0.75	0.5	0.02	0.57	0.03	0.47	—	0.02 C
Ash 35	Tyrol	1927.1427	End-winged axe	7.08	0.38	0.43	0.3	tr	0.12	0.03	0.12	0.06	0.11 D
Ash 1	Hallstatt	1927.947a	Embossed sheet	10.38	0.48	0.18	0.3	0.04	0.34	0.19	0.12	—	D
Ash 32	Hallstatt	1927.917	Bracelet	7.91	0.49	0.54	0.3	0.03	0.31	0.02	0.25	tr	0.18 D
Ash 2	Bernissart, Belgium	1927.1021	Razor	8.44	0.53	1.38	2.7	0.09	0.55	—	0.17	tr	C
Ash 5	France	1927.1022	Razor	7.17	0.13	0.08	0.2	tr	0.05	0.01	0.04	0.04	C

Analysis of European material in the Charlottenburg Museum, West Berlin

Ber 1	Thüritz, Kr. Salzwedel	II.1536	Blade	4.89	0.44	0.71	—	0.03	0.59	0.03	0.24	0.07	—
Ber 2	Central Europe		Median-winged axe	6.82	0.60	0.62	0.1	0.03	0.48	0.04	0.05	tr	—
Ber 3	Central Europe		Sword blade	5.81	0.05	tr	—	0.02	0.21	0.02	0.08	tr	tr
Ber 4	Central Europe		Median-winged axe	4.53	0.89	0.90	0.2	0.02	0.51	0.01	0.39	tr	tr
Ber 5	Kalbe	II.41516	Knife	12.22	0.26	0.79	0.4	0.03	0.26	0.01	0.44	tr	tr
Ber 7	Höver, Kr. Uelzen	II.9605	Sword blade	3.33	0.44	1.19	3.4	0.04	0.39	0.02	0.42	0.06	—
Ber 8	Sundmark	IIa.3348	Sword	7.96	0.05	tr	0.1	0.01	0.32	0.02	0.08	tr	—
Ber 9	Sorup, Kr. Sønderburg	II.9609	Sword	9.25	0.17	tr	0.1	0.02	0.41	0.02	0.03	—	tr
Ber 10	Neebelsen, Kr. Sulingen	I.1.1571	Sword	7.38	0.42	0.31	—	0.15	0.42	0.50	0.06	0.07	tr
Ber 11	Roesen, Kr. Jerichow II	II.10678	Sickle	5.92	0.10	0.06	0.1	0.02	0.19	tr	—	tr	tr

What of Hallstatt D and the early Iron Age? Analyses are still too few and scattered but what has been done is sufficient to show that the application of the same approach as used in Bronze Age metallurgy will yield immense dividends. There are clear signs that it is still possible to identify regional structures based on compositional and technical variations. As yet there is no clear indication of the preferred type of alloys. One interesting suggestion to emerge (Northover in Cunliffe, 1982) is that the copper and tin of Cornwall and Devon first achieved a more than local significance, possibly in conjunction with the manufacture of sheet bronze.

This paper has attempted to show the utility of bronze analysis in a wide range of prehistoric contexts and to demonstrate the ways in which it is possible to plot the movement of bronze on a large scale. This utility is much enhanced by a very detailed examination of typological change in relation to metallurgical change and by the examination of as large a body of data as possible. For the various purposes of the current bronze analysis project in Oxford some 1300 analyses have been completed in a little over two years and this is the sort of scale which must be contemplated in any project of this nature. At the same time great attention is being paid to the recovery of process information from the bronzes themselves and by the experimental simulation of manufacturing processes and the determination of the properties of the alloys used in a prehistoric context. These results have already had a major impact on our interpretation of Bronze Age metallurgy. One conclusion of general import should be mentioned here: we have seen a large variety of alloy types in the course of our analyses but process variables probably have a greater impact on the final behaviour of an implement than the alloy used. For instance it is impossible to cold work metal cast in clay moulds without homogenisation whereas this is perfectly feasible with metal cast in metal or stone moulds for a wide range of leaded bronzes (Northover and Staniaszek, 1982); this must have been one of the factors encouraging the differentiation between tool production (metal/stone moulds) and weapon and other production (clay moulds) in the later Bronze Age. We are also examining mathematical models of bronze production in an attempt to determine both the appearance of the metal supply to and from a Bronze Age foundry and the ways in which the archaeological record of surviving bronze might have been formed. The ultimate aim is a view of Bronze Age metal production and distribution that is both archaeologically and metallurgically realistic.

Abstract

It is argued that work on prehistoric bronze metallurgy in Europe has been inhibited by undue concentration on establishing a direct link between ore sources and the metal produced. Modern techniques such as electron probe microanalysis can give a great amount of information about such things as changes in source of supply and in metalworking techniques. Experience has shown that the number of metal sources used at one time was very limited, and there was often only one. The concept of a *metal circulation zone* is then defined and a review of the development of metal working in prehistoric Britain is made in the light of the currently available analyses, particularly

THE EXPLORATION OF THE LONG-DISTANCE MOVEMENT OF BRONZE

the large number recently carried out at Oxford. Finally, it is suggested that process variables have a greater effect on the behaviour of an implement than the alloy used.

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The Relevance of Chemical Provenance Studies to Celtic Ironwork in Britain

by C. J. SALTER

There are two important differences between the production methods used to produce early copper-based and iron-based alloys, which make the study of the chemical compositions of iron objects very much more difficult than those of copper objects. The first of these differences is that, copper and its alloys are liquid at temperatures below 1,100°C, whereas pure iron does not melt until 1,534°C. The primitive furnaces used in Britain during the period under consideration would not have been able to reach temperatures as high as 1,500°C in normal use. The melting point of iron alloys could have been reduced by the addition of alloying elements such as carbon, but this would have required the metal being held in contact with charcoal at high temperatures for a prolonged period. There is evidence of this having occurred accidentally during the Roman period. However, the cast iron produced appeared to have been rejected by the metalworkers as waste, probably due to its brittle nature (Tribbick, 1974). Thus, the iron in the Early Iron Age artefacts would have remained solid throughout the manufacturing process and would not have been homogenised by mixing, unlike a liquid copper alloy. The inability to melt the metal meant that it was more difficult for the smelter to remove the unwanted gangue elements from the metal, which is why a high concentration of slag inclusions is often seen in the iron of the Early Iron Age.

The second complicating factor in interpreting the compositions of iron artefacts, as compared with the interpretation of the composition of copper alloys, is the different geological distribution of the two metallic elements. Copper ores are, normally, only found in a few relatively restricted geographical locations, usually associated with some form of igneous activity, such as the granites of the south-west peninsula. On the other hand, iron ores are widely distributed throughout England and Wales, associated with a number of different geological formations. Many of these deposits are of a local nature, and would not now represent an economic ore deposit. Thus, little tends to be known about their chemistry. At the same time the ores which are at present, or have recently been, economic are often of low quality ores with iron content so low that they could not have been smelted in the Celtic smelting furnaces.

The geochemistry of iron is further complicated by the relative mobility of the element under the action of the atmosphere. Iron can be leached from the primary deposit and can be redeposited as a rich secondary sedimentary ore. An example of

this appears to have occurred at Claxby in Lincolnshire, where large quantities of Roman smelting slags are to be found down on the sandy alluvial soils 1–1½ miles from the outcrop of the Claxby ironstone on the scarp edge (Judd, 1870). However, at the site of the slag heaps there was also a source of iron ore. This ore was in the form of hard pan concretions 30–45 cm thick of a high-grade ore suitable for bloomery smelting. Similarly, there are areas of the chalk and the Jurassic clays where iron sulphide nodules are relatively common. Near the surface they can be naturally converted into a high-grade ore, or they could be roasted to remove the sulphur to produce a useable ore. It is, however, unlikely that such areas of scattered iron sulphide nodules would have been able to produce sufficient ore for the large-scale or prolonged production of metal. They almost certainly could have produced enough metal for immediate local use. It is interesting to note that distributions of iron sulphide nodules have been discovered at the hillforts at Danebury and Hambledon Hill. The numbers of nodules collected on these sites was rather greater than that one would expect from the geological settings of these sites. This suggests that iron sulphide nodules were being brought to these hillforts for some purpose.

As well as the problem of identifying the possible iron ore sources in an area there is the added complication that certain sedimentary ores can have considerable variations in their chemistry, due to local changes in the conditions of deposition. Thus, the chemical analysis of iron artefacts was ignored for the following reasons:

1. The ready availability of local iron ores would remove the need for the long-distance trading in semi-finished iron.
2. The variability of the chemistry of the ore bodies was likely to make it impossible to distinguish one ore from the next.
3. The inhomogeneity of the metal was likely to lead to large variations in composition in the same piece of metal.
4. The non-uniform distribution of slag inclusions which concentrate those elements rejected from the metal during the smelting operation, would cause sampling problems.
5. The unknown effect of recycling scrap metal on the composition of the iron and inclusions.

Up until 1970 most analyses of iron artefacts were from a few choice objects from each site or collection of artefacts which were likely to be metallurgically interesting. Usually, only a few elements were analysed, typically carbon, phosphorus, sulphur, and then maybe silicon and manganese. The analysis of the last two elements was, however, virtually meaningless as these elements segregate strongly to the slag inclusions.

In 1970, Haldane (1970) reported the analysis of 29 objects from eight Early Iron Age (EIA) sites around the Somerset levels. The stated purpose of the study was to answer the question as to whether the production of iron was local to each site, or whether there were a few specialists supplying the worked and unworked iron over a large area. The result of the linked cluster analysis performed on the data was that there proved

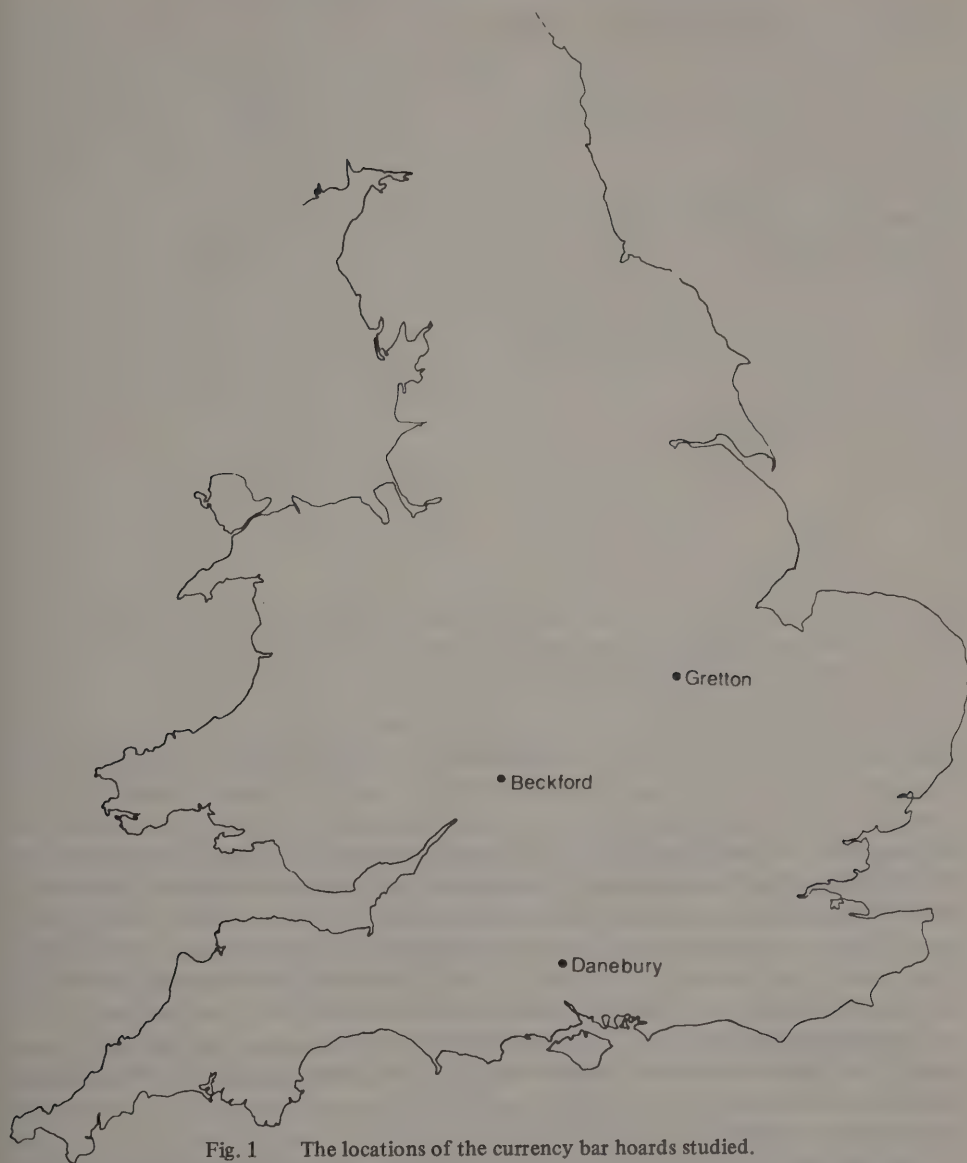


Fig. 1 The locations of the currency bar hoards studied.

to be a strong correlation between the provenance of the metalwork and the trace element composition.

Later work by Hedges and Salter (1979) studied a single artefact type, the long sword-shaped 'currency' bars, which have been found over a wide part of southern England (Fig. 1). The 'currency' bars analysed in this study came from Danebury in

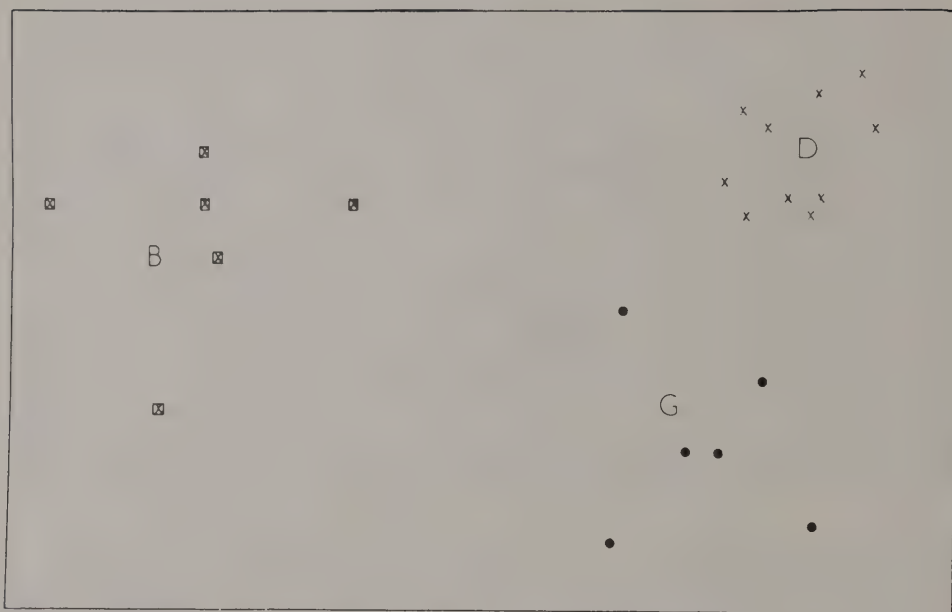


Fig. 2 The results of the discriminant analysis on the results from the analysis of the Beckford (B), Danebury (D), and Gretton (G) currency bars.

Hampshire, Gretton in Northants, and Beckford in Gloucestershire. The analysis method used was electron probe microanalysis (EPMA) rather than atomic absorption which Haldane had used. Electron probe microanalysis was thought to have the advantage that the metal and the inclusions could be analysed separately, also that the specimen can be retained for further metallography. Each sample was analysed at three or four points, with 15 elements being analysed in the slag inclusions and six elements in the nearby metal. Discriminant analysis was carried out on the averaged results from each sample. The results of this analysis are illustrated in Fig. 2, where it can be seen that the method has distinguished between artefacts from the three different sites. The morphology and microstructures of the bars also suggested that the bars were not manufactured at the same place. The Danebury bars were in general much thinner than those from Gretton, whereas the Beckford bars had lower inclusion densities and higher carbon contents than the bars from the other two sites.

One archaeological conclusion that could be drawn from these results was that the metal in these hoards did not come from a single site. On the other hand the tight grouping of the Danebury analyses suggests that these bars were manufactured from iron obtained from the same source or at least a number of very similar sources.

More recently this type of study has been extended by examining objects from a single site, Danebury, in more detail (Salter, forthcoming). The study has, so far, involved

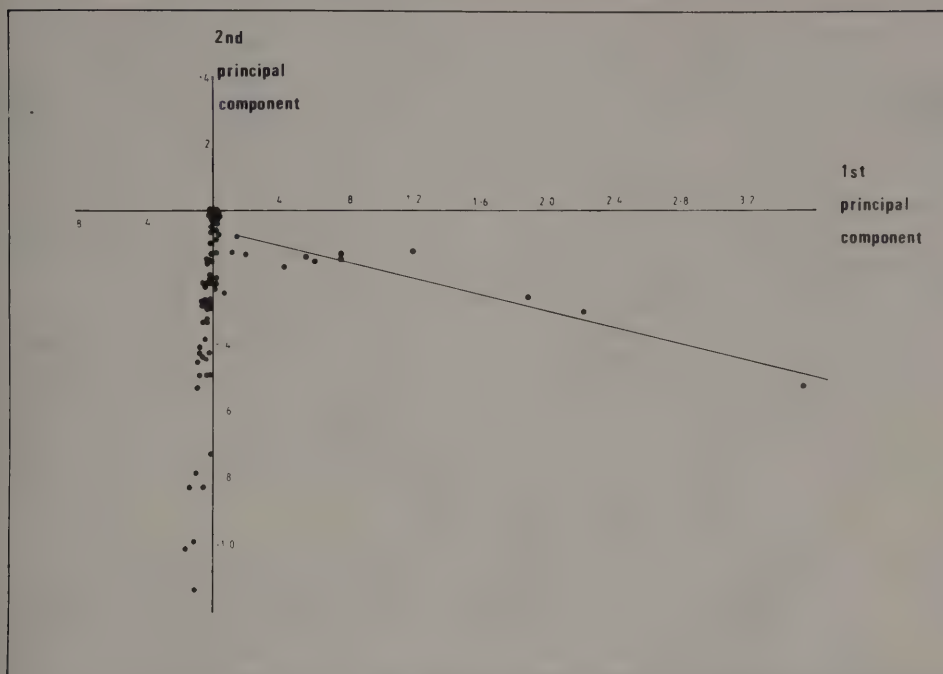


Fig. 3 The plot of the first and second principal components for the elements Co, Ni, Cu, As, P, S for the Danebury artefacts.

the sampling of 80 objects from the total of 234 iron artefacts found on the site and the analysis of 420 individual points. The purpose of this study was to determine whether there was any variation in the composition of the iron artefacts, with either time or artefact type, or indeed if there was any significant variation in the chemical composition of the artefacts found on a single site.

The variations in the inclusion compositions, in general, proved to be complex and as yet are not fully understood. However, there were some artefacts which could be characterised by the concentration of relatively few elements. The distinctive elements were cobalt and nickel in the metal and manganese in the inclusions. Fig. 3 shows the principal component diagram for the elements measured in the metal, i.e. cobalt, nickel, copper, arsenic, phosphorus and sulphur. The line marked indicates the trend of the high cobalt–nickel group of compositions. It can be seen that towards the left hand end of this line the group cannot be separated totally from the main compositional group. This lack of separation was due in a large part to the fact that each point marked was the result of the averaging the results from several points on each sample, in which there was more than one metal type present.

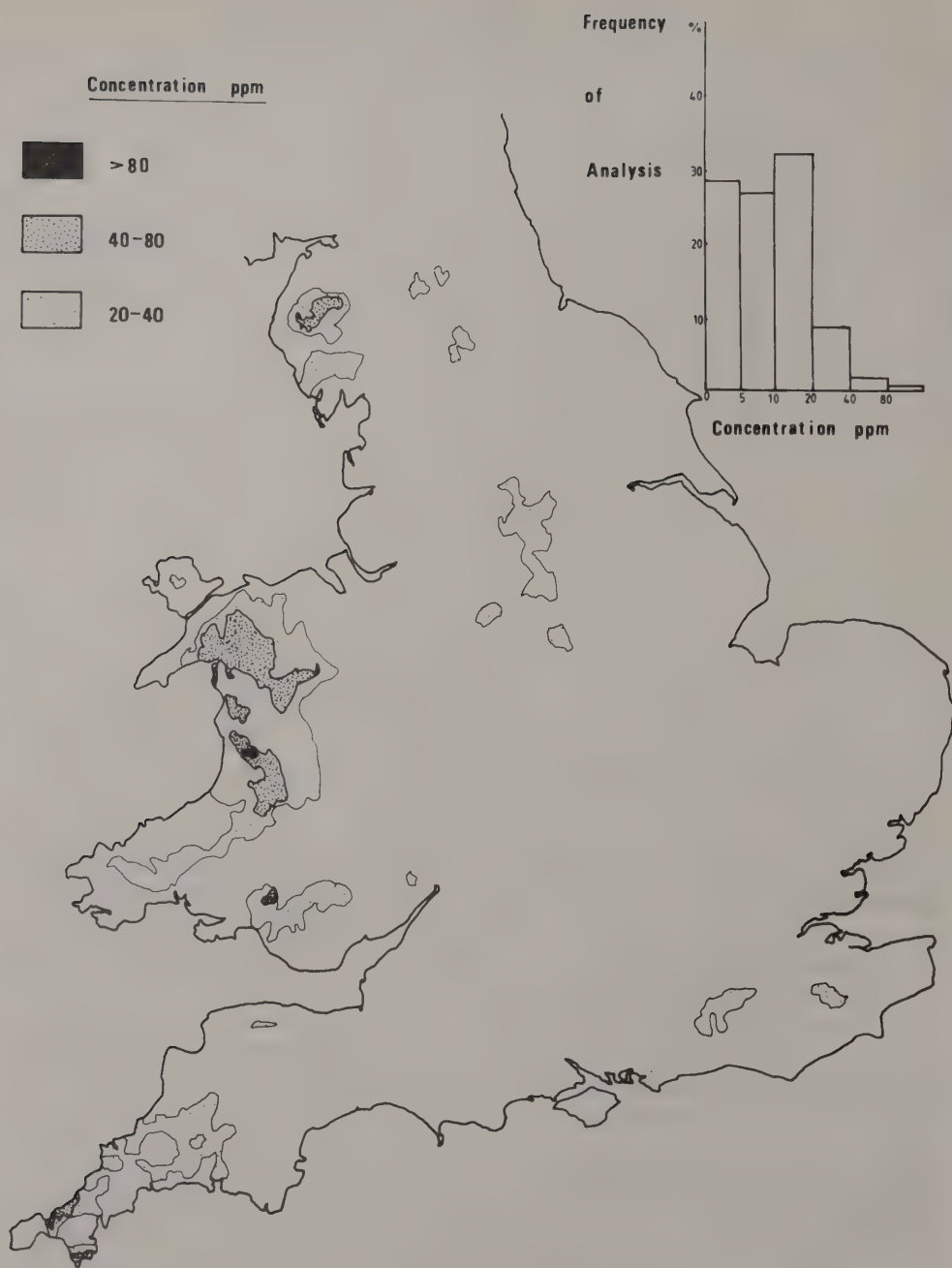


Fig. 4 The distribution of cobalt in stream sediments (after the *Wolfson Geochemical Atlas of England and Wales*).



Fig. 5 The distribution of nickel in stream sediments (after the *Wolfson Geochemical Atlas of England and Wales*).

Once a distinctive metal type has been indentified, the next question to be asked is: where was the ore mined that was used to produce this metal? Here at present it is necessary, unfortunately, to enter the realms of speculation, but speculation helped by the *Wolfson Geochemical Atlas of England and Wales*. The areas which one would expect to produce such an ore would be those which have enhanced concentration of nickel, cobalt and manganese. Figs. 4 and 5 show the distribution of these elements in the stream sediments of Wales and southern England. The results of this geochemical survey must be used with care as the distribution of the elements can be disturbed by recent industrial activity: however, it can be seen that the area immediately around Danebury does not show enhanced levels of these elements. The regions of southern Britain that show enhanced levels of these elements of Co, Ni and Mn are the south-west peninsula of England and north Wales, and to a lesser extent the northern and western rim of the Weald. There is some evidence to point to a western source for this characteristic metal. An analysis of a nail from Glastonbury by Haldane (1970) had a composition which would fit on the midpoint of the trend line indicated on Fig. 3. A very similar analysis has also been recorded from the blade of the Standlake sword (unpublished analysis by the author).

In summary, although there are many problems associated with the process of correlating the compositions of iron artefacts with those of the ores that could have been used to produce the metal, it has been possible to identify a distinctive metal type. Indeed, a metal type that is sufficiently distinctive that it should be possible to relate it to the ore of a specific region, if not a single ore body. The presence of the same type of metal at a number of different locations would seem to suggest that there was at least some limited long distance movement of iron in Britain during the Early Iron Age, even though the country was well endowed with iron ores. Unfortunately, the number of iron analyses from the Celtic period is small, so that at present it is impossible to quantify the size and type of movement involved. It is hoped that by the analysis of objects from other sites, it will be possible to define the range of this metal type in both space and time.

Abstract

The widespread occurrence of limited quantities of iron ore and the variability of the metal in early iron artefacts has for long discouraged attempts to study this material systematically by means of the large-scale application of analytical techniques. Analyses were therefore generally confined to objects likely to be metallurgically interesting, and even then only a few elements were analysed. In 1970, W. Haldane, using analyses of 29 artefacts from sites in Somerset Levels area, demonstrated a strong correlation between the provenance of the objects and the trace element composition. Later, R. E. M. Hedges and the author using electron probe microanalysis, showed that currency bars from Gretton, Beckford and Danebury differed in microstructure (as also in morphology) and that the metal therefore came from different sites. The tight grouping of the Danebury

THE RELEVANCE OF CHEMICAL PROVENANCE STUDIES

analyses suggested that the iron used may have been from a single source, or a number of very similar areas. Further work on the Danebury material has shown the variability of the inclusions, but some artefacts contain a concentration of elements in the metal and inclusions which suggests that the ore used must have come from Wales or certain areas of southern England.

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Hallstatt D Daggers: Britain and Europe

by E. M. JOPE

Many fine pre-Roman Iron Age daggers have been taken from the bed of the Thames along its lower and middle reaches (Jope, 1961; Macdonald, 1978). Among these is a fine weapon from Mortlake, complete in its sheath, of Hallstatt D-1 type (early to mid-6th century BC), now in the Museum of London (Plate Ia, b; Jope, 1961, pls. XVII–XVIII). This weapon has not been noted in recent discussions of Hallstatt-D daggers (Rieth *et al.*, 1969; Sievers, 1980), but it is of some importance as it brings Britain immediately into the cultural ambit of 6th-century Europe with more clarity than almost any other object.

The Mortlake dagger has a complex hand-grip, which carries us straight to Austria, as it invites comparison with that of the short sword in grave 555 in the cemetery of Hallstatt itself (Kromer, 1959 pl. 115). The swelling spindle-shaped hand-hold has vertical grooves or channels stopped at top and bottom with six lateral grooves (Fig. 2; the Hallstatt weapon has seven and nine lateral grooves pl. 16). These spindle-shaped hand-holds were made in two parts, of two half spindles of thin iron sheet (c. 0.5 mm) joined along the vertical seams by hard solder (i.e. containing a high proportion of copper: Hundt and Eichhorn in Rieth *et al.*, 1969: 57). On the Mortlake dagger these coppery vertical seam-lines had been misinterpreted as implying an inner lining spindle sleeve of bronze (Jope, 1961: 329 and pl. XVIII). On some European hand-grips of this spindle form the grooves have been filled with hard solder (Rieth *et al.*, 1969), but no trace of such metal has been found in the grooves of the Mortlake dagger hand-grip.

This hand-grip has a slender iron bar across the top, jointed into the tang; this pommel-bar is slighter than those seen on the European analogues (Rieth *et al.*, 1969: figs. 2 and 7). Both it and the hilt-guard have holes in their outer ends to carry stems for the usual knob-finials, as in European usage, but it shows no evidence of any holes to carry settings across the top (cf. Rieth *et al.*, 1969: figs. 2, 3, 9, etc.).

The relations of the sheath are rather different. The top member of iron is slightly curved across its upper surface, to fit exactly the curve of the hilt-guard. It carries four flat bi-cones of bronze on its front face (plain on the back), which immediately brings to mind the small group of broad-bladed daggers in the Bavaria–Swabia area (especially Etting, Weilheim, 35 miles south-west of Munich; Reith *et al.*, 1969: fig. 3; our map, Fig. 1; Sievers, 1980: fig. 4). And we see that the spindle hand-grip is also most frequent in this Swabia–Bavaria area (map, Fig. 1; Sievers, 1980: fig. 4), though in Europe these



Plate Ia Iron dagger in bronze-bound wooden sheath, from the Thames at Mortlake (no. 1). The holes for the twin suspension loops are seen towards the top of the back face (right). Scale: just under 1:2. (Photos: London Museum) (Jope, *PPS* XXVII, Plate XVII)

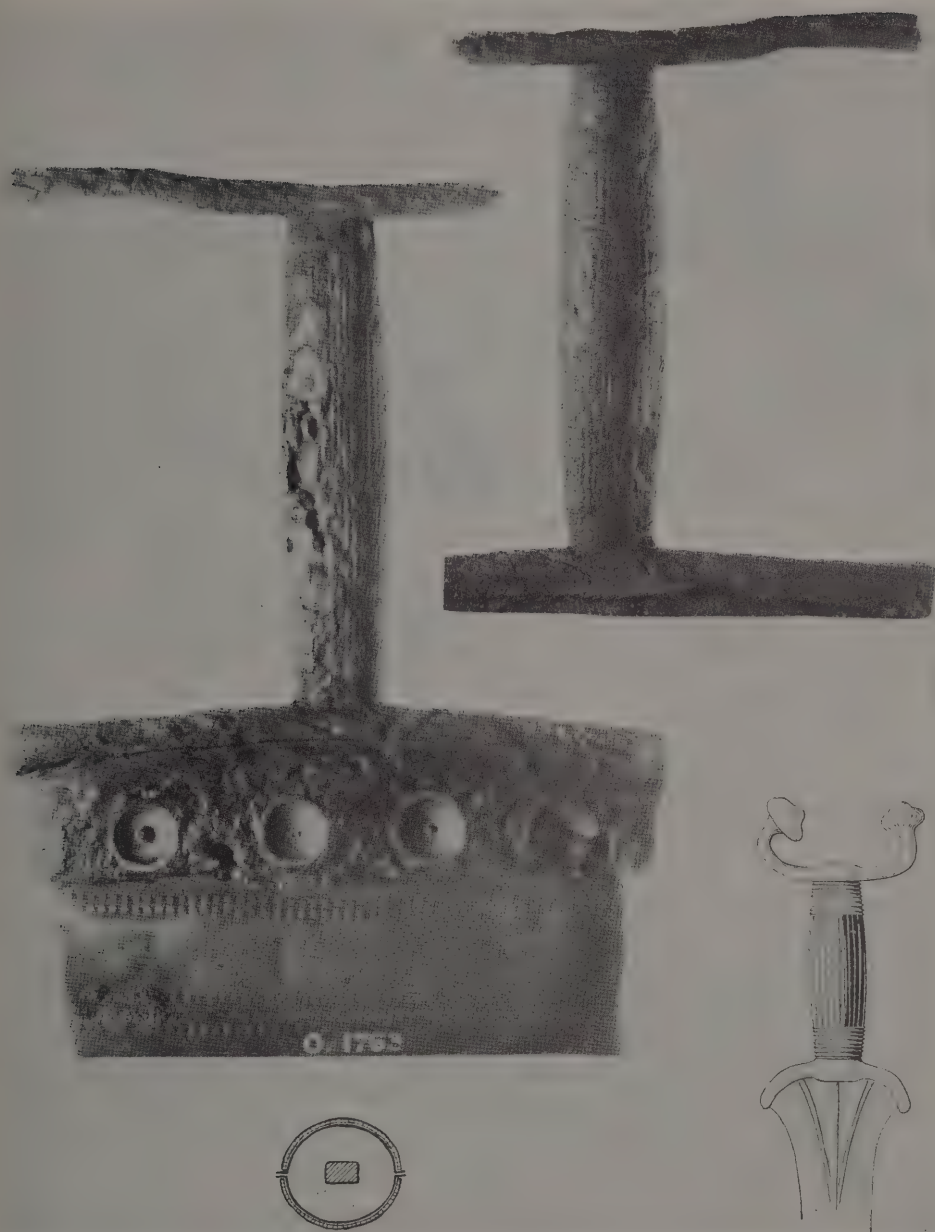


Plate Ib Dagger from Mortlake (no. 1): detail of handle. Natural size. Below right: Hilt of antenna-sword from Hallstatt, grave no. 555. Scale 1:3. Below, left: Inferred section through hand-grip of no. 1 showing iron tang, wood core, covered with swelling two half-cylinders of thin iron which carry the scored lines. (Photos: London Museum) (Jope, *PPS* XXVII, Plate XXVIII)



Fig. 1 Map illustrating some connections between South Britain across Temperate Europe in the 6th to early 5th centuries BC: iron daggers and bronze cordoned buckets with swing-handles. ▽ Daggers with composite spindle-shaped handgrip (as from Mortlake and Hallstatt grave 555, fig. 2). ▴ Other relevant broad-bladed daggers: with bi-cone ornaments: ▴ on sheaths ▴ on pommel. • Cordoned swing-handle buckets, type as from Weybridge (Brooklands, Surrey) and Kund, Hungary (after Stjernquist, with additions. Cordoned buckets of closely related variant types. LN, London (Mortlake); W, Weybridge (Surrey) Ha, Hallstatt, Austria. The concentrations of black in the Swabian area of the Upper Danube suggests the production area of daggers with these features.

two features are not seen in use together on the same weapon. Our short sword from Hallstatt grave 555 thus appears moreover to be an outlier of the spindle hand-grips, with the production centre some 200 miles to the west in Swabia.

Below this top band the character of the Mortlake sheath changes abruptly. The succession of thin bronze bands wrapped round the blade shape is a manner little seen

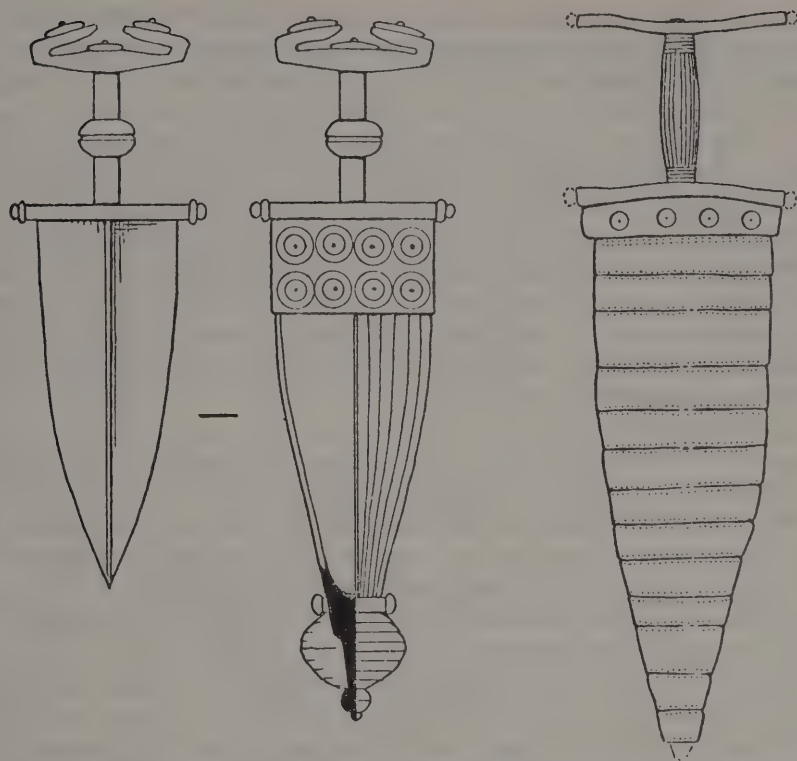


Fig. 2 Iron daggers of the Ha-D type, from the Thames at Mortlake, London (right), and Etting, Kz, Weilheim, Bavaria (centre and left). No evidence survives for the chape construction of the Mortlake dagger. ($\frac{1}{4}$) (Jope, *PPS* XXVII, Fig 1)

in Europe (but note the Magdalenenberg grave 39, Spindler, 1972; cf. Jope 1961: fig. 2.3 and pl. XIX; Macdonald, 1978). The manner in which these bands are hitched up the back into a succession of fold-over joints gives a somewhat rustic aspect (Jope, 1961: pl. XIX), and the double loop suspension system was an entirely British development (Jope, 1961: 307 ff.), the one example known from the continent being most probably a British export (Marien, 1963; Macdonald, 1978).

There seems now to be no means of knowing the nature of the chape on the tip of the Mortlake dagger, but it was probably rather insecurely fixed to the wood sheath-lining; the chape is likewise gone on daggers of analogous construction (Jope, 1961: 310, pl. XIX; Marien, 1963; Macdonald, 1978; Spindler, 1972). The Mortlake dagger was wrongly given a globe chape in Jope, 1961: fig. 1.

The Mortlake dagger is thus a composite weapon, and brings together a wide range of Hallstatt-D daggermakers' traditions, skills and techniques, from middle Europe and southern Britain. If this weapon and its sheath were throughout entirely the work of

craftsmen working in Britain it was indeed a *tour-de-force*. A more acceptable interpretation might seem to be that the original dagger in its original sheath was initially a product of an armourer working the Swabia-Bavaria area, and that this weapon was brought to Britain, perhaps as a present between chieftains rather than by mercantile means. Subsequently the original sheath became damaged and had to be renewed; in this operation a Thames valley armourer would seem to have retained the top member of the original sheath (or of the sheath of yet another dagger or European origin) with its bi-cone settings, and made a new sheath on a different constructional system based on wrapped bronze bands, and incorporating a newly developed innovative British suspension system using two loops. Here then would be our only known imported pre-Roman Iron Age dagger; it could even be the primary imported prototype initiating the whole British dagger series, a continuous development from the 6th century BC into the 1st century AD (Jope, 1961).

Other composite refurbished weapons of the Iron Age are known from the Thames: the Minster Ditch dagger (Jope, 1961: pl. XXII; Jope and Jacobsthal, 1984: pls. 18–19), and the Standlake sword (Jope and Jacobsthal, 1984: pls. 48–9).

Our dating for Hallstatt D-1 has been greatly sharpened in recent years by the cumulative work on tree-ring sequences, notably in an area that much concerns us, on the timbers from the Magdalenenberg burials of just this age (Spindler, 1972, 1980: 215 ff.; Hollstein, 1980; summarised in Hawkes, 1974), and we may perhaps expect collateral data from the British Isles in the not too distant future.

Authentic imports of the Iron Age from the European continent are rare enough in the British record but not unknown. Though this Mortlake weapon seems the only possibly imported Hallstatt-D dagger, there is the antenna sword from further down the Thames (Brailsford, 1953: fig. 23.1) and the Ha-D cauldron also (Hawkes and Smith, 1957). A generation or so later the fine cordoned bucket from Brooklands race-track near Weybridge was probably made in the region of Kurd, west Hungary, where 11 closely similar cordoned buckets probably indicate a cemetery (Stjernquist, 1967; Jope and Jacobsthal, 1983: pls. 10–11). Three centuries earlier there are the other 'Kurd'-type buckets (Hawkes and Smith, 1957), and we should not overlook the impressive bronze dagger with cast bronze hilt, a millennium earlier, also from the Thames (Brailsford, 1953: fig. 8.6). Further away to the west, the magnificent fleshhook from Dunaverney in County Antrim, Northern Ireland, should be treated with more reserve, for the fine swans, cygnets and ravens could be later additions to the main shaft, their stems passing through what may be secondary holes; the implement itself is in any case a native type, and the birds do not look like real Hallstatt ducks (Brailsford, 1953: pl. IV; Jope and Jacobsthal, 1984: pl. 9).

One further point should be made here. In Europe most of the daggers and other relevant weapons come from rich burials. In Britain we have virtually no such chieftains' burials of the Iron Age, and most of our daggers come from the bed of the River Thames or its tributaries, and this applies also to much other fine metalwork, such as the parade shields (Jope, 1976). The compelling inference is that some riverine burial

rite was customary among the people of the pre-Roman Iron Age in many parts of Britain (Joep, 1961), a striking contrast with the continental Celtic tradition of cemetery burial.

Abstract

A dagger, complete with its sheath, found in the bed of the Thames at Mortlake, is described in detail. The weapon is of Continental type, while the sheath has been repaired, probably in Britain, though the original top band, shaped to fit the hilt-guard of the dagger, was retained. A provenance in the Swabia-Bavaria area is suggested for the dagger and its original sheath, which may have come to Britain as a gift from one chieftain to another. It is the only known imported pre-Roman Iron Age dagger in Britain, one of the few authentic Iron Age imports, and it could be the prototype which originated the entire British dagger series from the 6th century BC to the 1st century AD.

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Rescue Archaeology in Sussex 1981: An Eighth Progress Report on the Sussex Archaeological Field Unit

by OWEN BEDWIN, DAVID RUDLING and ANDREW WOODCOCK

Introduction (Fig. 1)

The year 1981 was a transitional period for the Sussex Archaeological Field Unit. Of the four research projects established in 1976 (Drewett, 1977), three had been brought to a conclusion in 1980. The writing-up and editing which resulted from two of these (Neolithic and Bronze Age settlement and the Bullock Down multiperiod projects) meant that the Unit's Director, Peter Drewett, was unable to carry out any excavation during 1981. The third project to be wound up was the investigation into the origin of towns in Sussex; this had, in any case, proved somewhat difficult to justify, not because of the research design, but because appropriate sites were not available on a rescue basis.

The fourth project (on Iron Age settlement) was not wound up, but was instead given a change of direction. A number of excavations carried out within this project during 1976–80 had shown that large-scale excavation on ploughed Downland sites



Fig. 1 Excavations undertaken by the Sussex Archaeological Field Unit in 1981: 1, Halnaker Hill; 2, Selsey; 3, Southwick villa; 4, Devil's Ditch, Boxgrove; 5, Selmeston; 6, Winchelsea; 7, Grey Friars, Lewes.

was providing diminishing returns. Equally, however, work at North Bersted (Bedwin and Pitts, 1977) and Oving (Bedwin, 1981a) had demonstrated, for the first time, the presence of extensive late Iron Age settlements on the Coastal Plain. Consequently, the Iron Age project has been transformed into one with the overall research aim of studying the development of settlement on the Coastal Plain from 1000 BC to AD 1000. This project, jointly directed by Owen Bedwin and David Rudling, was in fact the only field project to be funded by the Department of the Environment in Sussex for 1981. Three excavations were carried out within this project; those at the Southwick Roman villa, the Devil's Ditch (part of the Chichester Dykes) at Boxgrove, and at Selsey. In addition, a number of other rescue excavations were undertaken outside the project; these were in the medieval town at Winchelsea, the small hill fort on Halnaker Hill, at Selmeston, and at the site of the historically documented Friary in Lewes.

1. Excavations at Halnaker Hill, Boxgrove, West Sussex

by OWEN BEDWIN

The hill fort on top of Halnaker Hill consists of a feeble, oval earthwork, 200 m by 120 m. The area enclosed is *c.* 1.5 ha, and there appears to be a single entrance to the north-west (Fig. 2). About 60% of the enclosure is ploughed annually and, with the aid of a grant from the Sussex Archaeological Society Margary Fund, a small excavation intended to date the earthwork was carried out in September 1981.

One ditch terminal was fully exposed (Fig. 3 and Plate I); if typical, it would suggest that the ditch was constructed as a series of 'linked pits'. The flintwork and very small amount of pottery from the lower ditch silts suggested a Neolithic date. A little animal bone (cattle, pig and sheep) was also recovered from these lower silts, and will be used to obtain a radiocarbon date. Soil samples were taken from the ditch silts for molluscan analysis.

The ditch on the other side of the entrance was sectioned by a trench 1.5 m wide (Trench B in Fig. 3). The ditch here was 2.0 m deep, and much of the lower silt consisted of a mass of very loose chalk rubble (layer 6 in Fig. 4), which is most plausibly interpreted as collapsed bank material.

In neither trench was there any indication of the survival of the original material forming the bank, nor any sign of post-holes from revetting the bank or from a gateway structure. The bank was almost certainly a simple dump.

Little insight into the function of the site was provided by the excavation, though it can perhaps be associated with the rather similar enclosure at Bury Hill in West Sussex (Bedwin, 1981b); i.e. it was possibly a site connected with ritual or ceremonial. Below Halnaker Hill, on a broad, south-facing slope, is a substantial field system (Fig. 5). The lynchets are so far undated, but it is hoped to excavate here in the near future to see whether or not they are contemporary with Halnaker Hill.

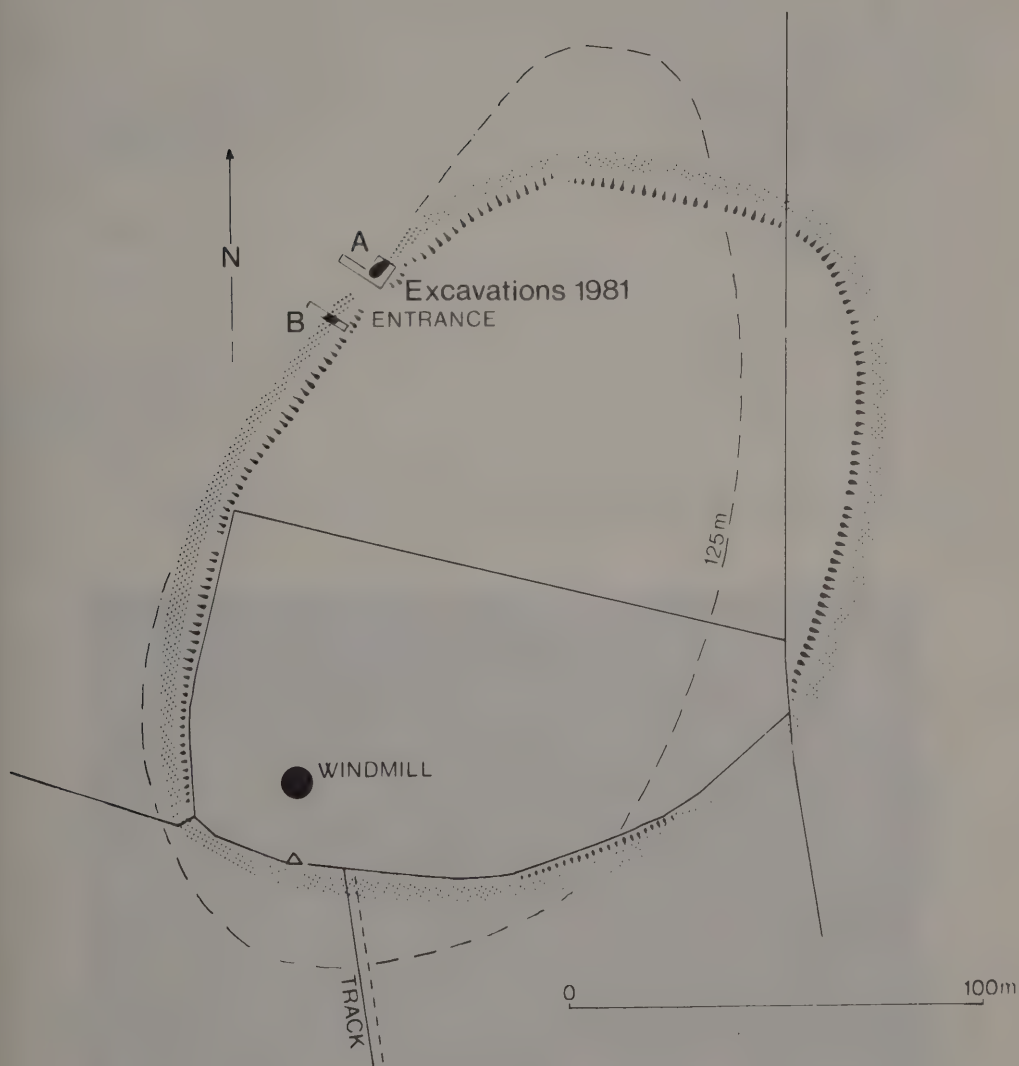


Fig. 2 Halnaker Hill, 1981. General site plan (survey by F. G. Aldsworth).

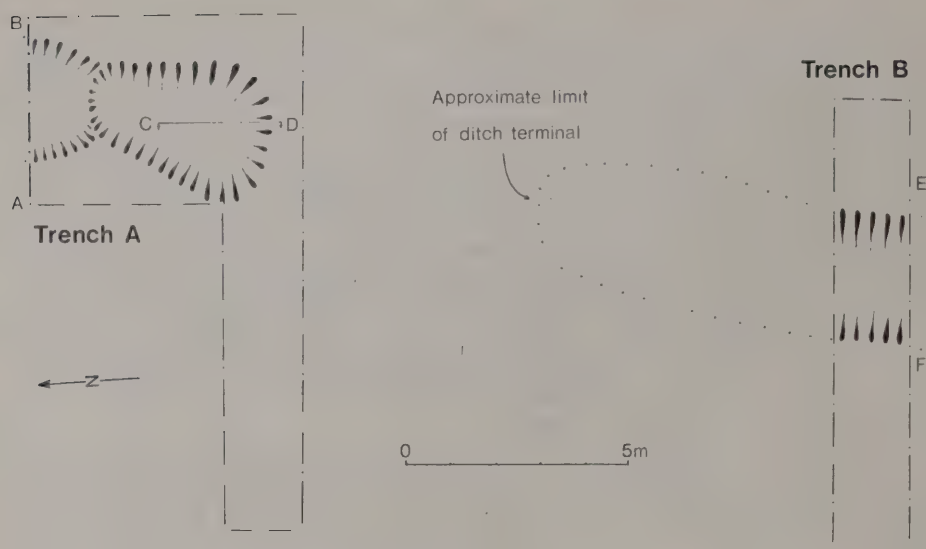


Fig. 3 Halnaker Hill, 1981. Plan of the excavations at the entrance.



Plate I Halnaker Hill, 1981. Northern ditch terminal from the south. Scale 2 m.

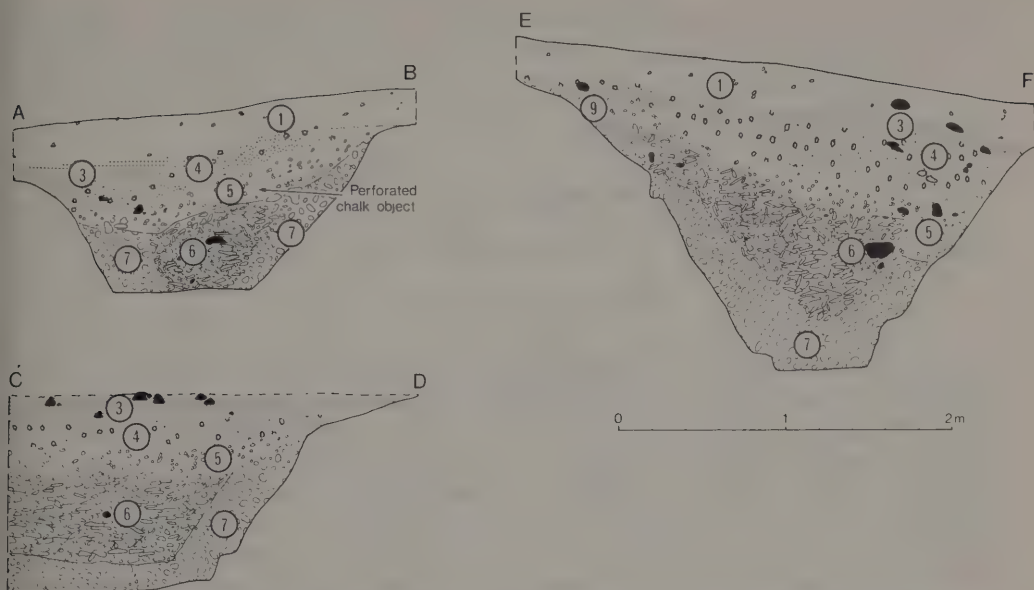


Fig. 4 Halnaker Hill, 1981. Sections through the enclosure ditch. For location of these sections refer to Fig. 3.

2. Excavations at the Byways, Selsey, West Sussex

by OWEN BEDWIN

Over the last 100 years, a wide range of material has been eroded from the coastline at Selsey. In particular, there have been major finds of Iron Age, Roman and middle Saxon date. In the case of the Iron Age, the large number of finds (including many coins) has led to the suggestion that there was an oppidum, now largely destroyed by erosion, near Selsey Bill.

To the rescue archaeologist, the area represents a considerable problem. Finds have been made both to the east and west of Selsey Bill, over a distance of several kilometres, and consequently a substantial stretch of shoreline is of potential interest. However, due to the rapidity of erosion, much of the coastline, especially to the east of Selsey Bill is protected by a concrete sea wall. Furthermore, almost all the land immediately behind the coastline is built on, and therefore opportunities for excavation are limited. In June 1981, it was possible, however, to carry out trial trenching on a coastal site (the Byways), in advance of house building. Roman material had been found on the foreshore immediately to the south of the site, but two trial slots were both barren of

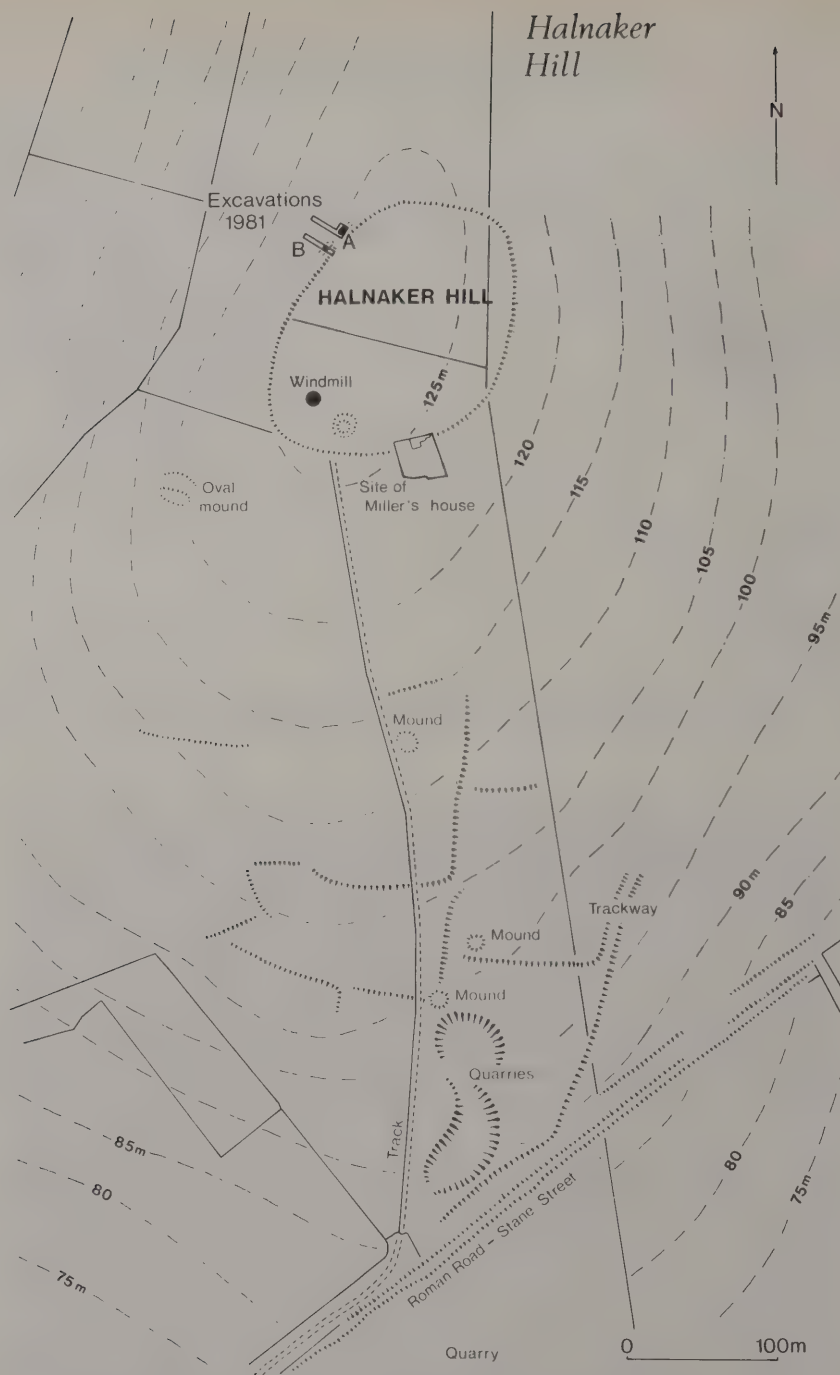


Fig. 5 Halnaker Hill, 1981. The hill fort and nearby field systems and barrows (survey by F. G. Aldsworth).

archaeological features, and only a sherd or two of Roman pottery came to light. In spite of this negative evidence, the Unit will continue to investigate sites at Selsey as and when opportunities arise.

3. Excavations on the Site of the Roman Villa at Southwick, West Sussex

by D. R. RUDLING

One of the most striking features of the Roman settlement pattern in Sussex is the appearance before the end of the first century of a group of large villas, 'a phenomenon which distinguishes the Regni from most other tribes of Britain' (Cunliffe, 1973: 74). So far five examples of this category of villa are known; Fishbourne, Angmering, Southwick, Eastbourne and Pulborough. These villas appear to have been 'imposed' on the late Iron Age settlement pattern and are presumably a reflection of the favourable political and economic climate in the client kingdom of the Regni (Bell, 1976: 251). Only the villa at Fishbourne has been excavated on any scale under modern conditions (Cunliffe, 1971) and our knowledge concerning the later histories of the other villas leaves a lot to be desired.

The villa at Southwick was first noted in 1815 and was the subject of a large-scale excavation in 1931 (Winbolt, 1932) and smaller ones in 1933 (Ward, 1934-5) in advance of house construction. A further limited investigation was undertaken in 1965 prior to the erection of the Methodist church in Manor Hall Road (Canham, 1966). This year (1981), at the request of both the Department of the Environment and West Sussex County Council, the Sussex Archaeological Field Unit carried out a small excavation in the area to be disturbed by the building of an extension on to the Manse which is located to the east of the Methodist church in what had originally been the north-west corner of the villa's courtyard.

Although the area investigated this year was very restricted, it lay adjacent to Trenches VII, XI and XII of 1965 which yielded 'several dozen post-holes, and shallow pits, cut into the subsoil from a low level', this indicating to the excavator an 'earlier occupation of the site' (Canham, 1966: 281). It was thus hoped that our trench would find further examples of such features and produce finds which could date them. Unfortunately the area available to us proved to have been severely disturbed in modern times. One pit was in fact discovered at a low level, but although this produced only Roman finds, its base lay above soil containing modern material and it must thus be a relatively recent feature with residual Roman occupation debris in it. The finds from the trench in general included: a number of worked flints; pot sherds (mainly of the first and second centuries), pieces of tile, one small stone tessera cube, a couple of fragments of bronze, and oyster shells.

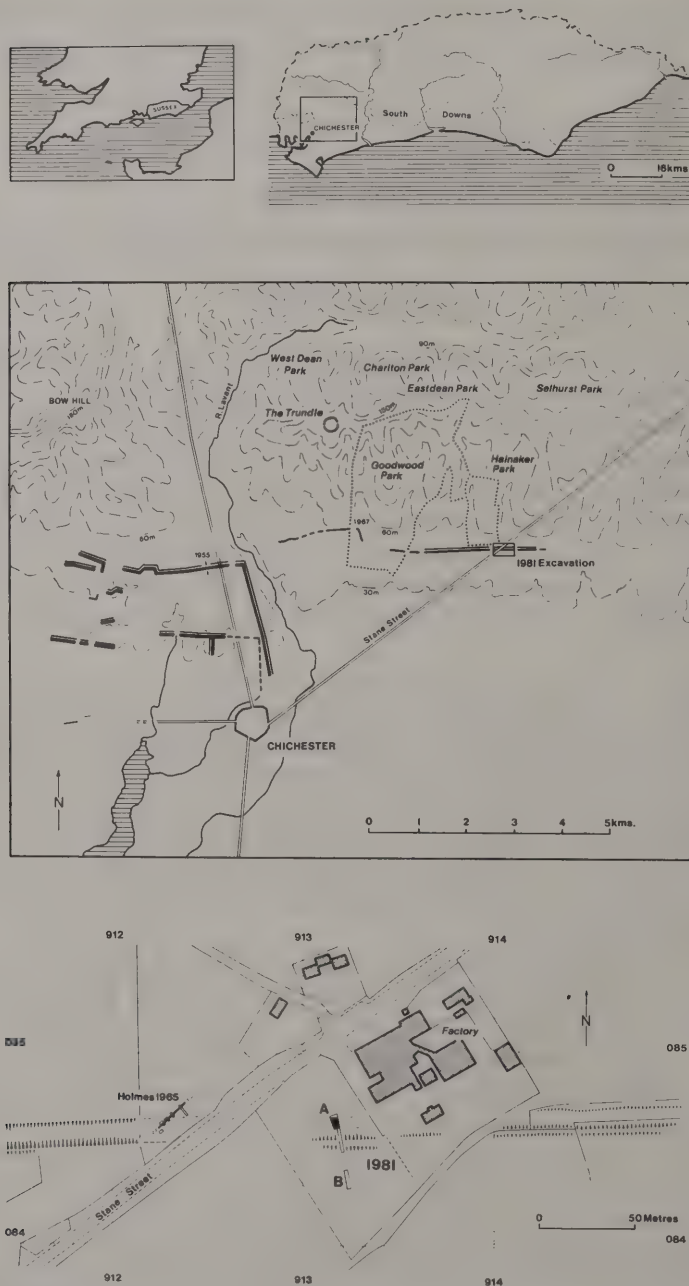


Fig. 6 Devil's Ditch, Boxgrove, 1981. Site location and general site plan (by F. G. Aldsworth).

4. Excavations at the Devil's Ditch, Boxgrove, West Sussex

by OWEN BEDWIN

The Devil's Ditch at Boxgrove forms the easternmost branch of the complicated linear bank and ditch system known as the Chichester Dykes or Entrenchments (Fig. 6). The earthwork was sectioned at SU 9132 0846 during October 1981, in advance of factory building. The bank at this point is rather feeble, and the ditch all but invisible, having been filled in completely.

The ditch (Trench A in Fig. 6) turned out to be 6 m wide at the top, 1.6 m deep, and with a wide, flat bottom, 2 m across (Plate II). This profile matches the one found by Holmes (1968) about 60 m to the west. However, the ditch excavated in 1981 contained much late medieval pottery and roof-tile, plus animal bone and oyster shell, to within 10 cm of the bottom. This probably represents domestic and building debris, perhaps from a medieval dwelling just to the north of the ditch, facing on to Stane Street (Fig. 6). It proved impossible to section the bank satisfactorily because of a large number of intertwining tree roots (Trench B, to the south of the ditch and bank, contained only a single post-hole, probably modern).

The finding of medieval pottery and tile throughout the ditch, almost to its floor, raises interesting questions about the origin of this part of the Chichester Dyke system.



Plate II Devil's Ditch, Boxgrove, 1981. Ditch section. Scale 2 m.

It has been widely accepted that the Chichester Dykes belong to the end of the Iron Age, and are to be thought of as a complex of defensive outworks protecting a large oppidum located somewhere on the Coastal Plain, perhaps at Selsey. Excavations at the western end of the Dykes in 1955 (Murray, 1956) and at Goodwood in 1967 (Bradley, 1971) have supported this (Fig. 6, middle). The results of the 1981 excavation do not appear to fit in with this, however. No re-cut was visible in the ditch section, nor was there any buried land surface (cf. Murray, 1956). If this earthwork is therefore of late Iron Age date, the ditch must have been carefully cleaned out in the medieval period, leaving no trace of Iron Age artefacts. The motivation for the refurbishment of the ditch could have been provided by its need to serve as the southern boundary of Halnaker Park (Fig. 6, middle and see also Holmes, 1968, for documents relating to this). Alternatively, this stretch of the ditch could be medieval in origin; it is noteworthy that the 1981 excavation revealed a ditch 1.6 m deep, with a wide, flat bottom, whereas, towards the western end of the Dykes, the ditch was 2.8 m deep with a V-shaped profile (Murray, 1956). Further work will clearly be necessary to decide between the two alternatives.

5. Excavations at Selmeston, East Sussex

by D. R. RUDLING

Selmeston has been an important area for settlement from the Mesolithic to the present day, and this year two sites were excavated prior to threats by building and tree planting.

The first area investigated was a building plot (TQ 511 071) to the east of the pond which lies to the north-east of the church. The site itself is situated on the clay near its junction with the Lower Greensand. Several test trenches were excavated but these produced no features and only a few struck flints and pieces of pottery. In contrast, one trench which was excavated nearby on the margins of the Lower Greensand near its junction with the clay produced several hundred struck flints and medieval pot sherds. These data help to confirm the idea that the medieval and earlier occupation of the region is likely to be concentrated on the Greensand.

The second site was an area of Lower Greensand which is located on the edge of the old sandpit which over the years has yielded considerable evidence for prehistoric settlement, including several Mesolithic hollows which were excavated in 1933 (Clark, 1934). The area investigated in 1981 is threatened by the continuing erosion of the face of the quarry and plans for tree planting to form a wind-break. Preliminary excavations undertaken in this area in 1978 (Drewett, 1979) following the discovery of a Neolithic bowl which had eroded out of the face of the sand pit (Drewett, 1975) revealed a spread of flint-making waste, three post-holes associated with large, unabraded sherds of middle-late Saxon pottery, and medieval field boundaries (the Saxon post-holes and medieval ditches are shown in Fig. 7, Trench I). The finding of this Saxon material was especially

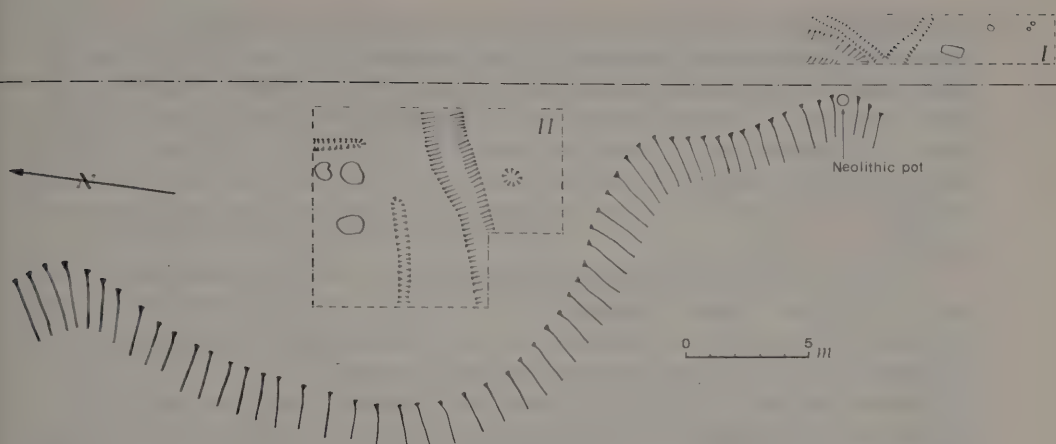


Fig. 7 Selveston, 1981. Position of Saxon and medieval features in trenches I (1978) and II (1981) in relation to the face of the sand pit.

interesting, and it was hoped that the recent excavations would lead to more discoveries of this period. Unfortunately this hope was not realised, but further traces of the medieval field boundaries were excavated, together with several small medieval pits (Fig. 7, Trench II). Again the excavations yielded a general scatter of Mesolithic and Neolithic flint flakes and tools, but no features were located which could be considered contemporary.

6. A Trial Excavation on the Site of the Grey Friars, Lewes

by D. R. RUDLING and A. WOODCOCK

Recent proposals to re-develop the derelict railway land in Lewes have meant that the site of a convent of Grey Friars is now threatened, and at the request of the East Sussex County Council the Sussex Archaeological Field Unit, in conjunction with the County Council, undertook a small trial excavation in order to assess the potential of the site for larger scale investigations.

The Lewes Grey Friars is known to have existed between c. 1230 and 1538, and that upon its dissolution it passed into lay hands. It was never a wealthy establishment (at the Dissolution its debts exceeded its disposable assets), or large. The site appears to have been on the Brookland margin of the river flood plain, outside the main inhabited area of the town. With regard to the later history of the site there are many references up to 1846 of a house called the 'Greyfriars' or the 'Friary', and this is said to have been built in 1673.

In 1929 a male skeleton was found when digging for gas mains in front of the Free Library and this was presumed to be an interment in the cemetery of the Grey Friars. Several other skeletons were said to have been found close by in Friars Walk many years earlier, and *c.* 30 yards away under the crossing into the railway yard. In 1967, Messrs J. Knight-Farr and D. Thompson undertook an excavation on the site of the Old Railway Station in Friars Walk, but unfortunately the discoveries were never published, although a sketch plan and photographs show that substantial ashlar foundations were uncovered.

This year's excavations were located on a small patch of wasteland which forms part of a car park situated immediately to the north of the area investigated in 1967. An 'L'-shaped arrangement of trenches was used, the north/south limb being 6 m long by 1 m wide, and the east/west limb 8 m long by 1.25 m wide.

On average about 1.2 m of modern deposits (dumping/ground levelling) had to be removed and unfortunately much of the east/west trench below this level had been destroyed by a concrete raft and the north-east corner of a cellar. The only other discoveries in this east/west trench were several brick walls and the apparently unmortared, chalk block footings of a wall running north/south. Unfortunately no dating material was found associated with this feature, but it is likely to be either medieval or early post-medieval.

Fortunately the north/south trench proved to be more rewarding and the main discoveries were the foundations of three parallel, chalk block walls (contexts 12, 22 and 25) with an east/west alignment (Fig. 8). Walls 12 and 22 yielded traces of an identical pebble mortar and are presumed to be contemporary. No mortar was discovered in association with wall 25 (which consisted of loose chalk blocks), and it is earlier than 12 or 22 (note it is sealed by layer 15). In between walls 12 and 22, and over wall 25, was a layer of crushed chalk (15). This overlies a layer of brown clay (23), which in turn overlies a layer of chalk (24), which unfortunately could not be excavated due to shortage of time. Possibly the various layers may have been necessary due to problems of flooding/the high water table in this region. Dating material from contexts 15, 23 and 25

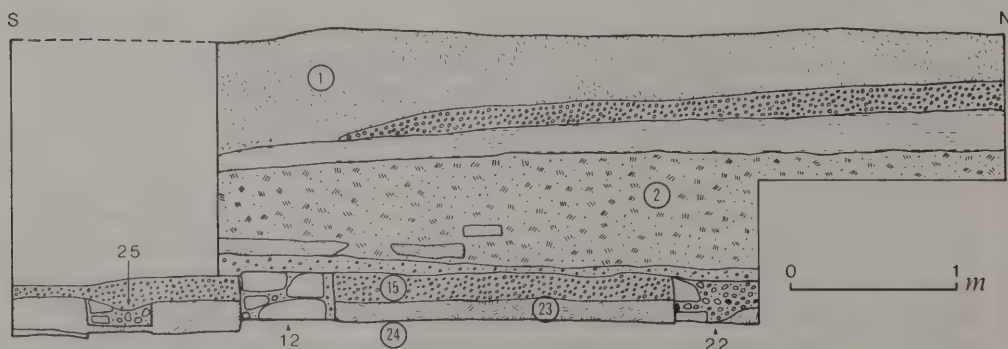


Fig. 8 Lewes, 1981. Section showing walls 12, 22 and 25.

show these to be medieval, and they are therefore presumably part of the Grey Friars complex. Wall 22 was cut by a well-laid, regular wall of mortared chalk blocks running approximately north-north-west/south-south-east. The type of construction of this wall suggests perhaps a 16th or 17th century date.

7. Excavations in Mill Road, Winchelsea

by D. R. RUDLING

In advance of proposed building work an excavation was undertaken on a small plot of land situated to the east of, and at present still belonging to, The Orchards, Mill Road, Winchelsea. In medieval times this land would have been part of Quarter III of Edward I's planned town of New Winchelsea (established between 1283 and 1292), and the main aims of the investigations in 1981 were to: further check the accuracy of Homan's plan of Winchelsea based on the rent roll of 1292 (Homan, 1949); to investigate examples of buildings and the methods of construction used from this quarter of Winchelsea, and hopefully also provide some indications as to the owner/s relative wealth and status in comparison with the other areas of the town which have already been sampled by excavation; and to obtain basic economic data (which were unfortunately very limited from previous excavations), and the still much needed pottery groups related to datable structures. Unfortunately it was not possible to investigate the street frontage since this is occupied (Fig. 9) by a very small, dilapidated, flimsily constructed cottage (of no real historical interest), which at present must be retained in accordance with the planning permission. The presence of the cottage makes the new building plot 'land-locked', and it was thus necessary to dig the site completely by hand, with (given the resources available) a consequential limitation on the size of the area investigated.

The excavations (Fig. 9) were aimed to sample both part of the rear range of any medieval house/s on the site and the garden area beyond (in the hope of finding useful rubbish pits, etc.). It quickly became apparent that there had been extensive destruction caused by later gardening, but that this problem was not quite so serious as in the case of the 1980 excavations in North Street (Rudling and Martin, 1981).

From the area immediately to the north and west of the 'cottage' came evidence for what appears to be a two-phase building. Belonging to the first phase were the clay-bedded sandstone foundations of a wall (context 6) running parallel to the street and this is presumed to be the rear wall of the medieval house. Possibly of similar date are the traces of a north/south sandstone wall discovered immediately to the west of the existing cottage. No definite traces of the front wall were revealed, and these presumably lie under the existing cottage. The second phase consisted of a room/building approximately 4 m wide and utilising medieval 'Flemish' bricks. The one-and-a-half brick wide footings of its western wall (121) were discovered adjacent to the possible west wall of the first phase building, while its eastern wall is marked by a band of pebble-lime mortar (73). In the

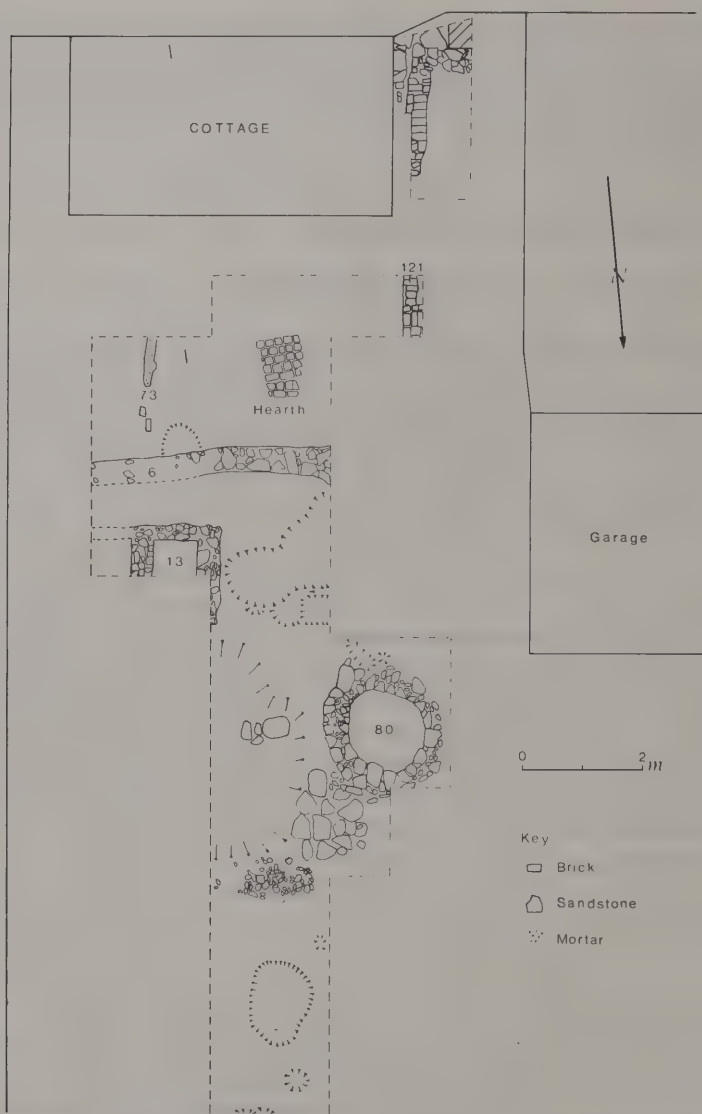


Fig. 9 Winchelsea, 1981. Trench plan showing the two phases of the medieval building and the main pits, etc., in the garden area to the north.

middle of this structure was a simple brick hearth, with the bricks laid flat as opposed to on edge, as was the case with the more elaborate hearths discovered in the tenement immediately to the north of Blackfriar's Barn (J. Bell, pers. comm.) and in North Street (Rudling and Martin, 1981). Data regarding the front wall are again lacking, as is information concerning the rear wall (possibly context 6 was re-used?).

In the 'garden' area to the north of the medieval buildings were a large number of pits, both medieval and more recent (not all are shown in Fig. 9). The most interesting of these was a large, stone-lined cess-pit (80), but unfortunately due to shortage of time and resources it was not possible to completely excavate this. The latest fill however dates to the late 16th or 17th century and yielded large quantities of bones, oyster shells, pottery, nails, other metal objects, and a bone chess-piece. The original date of construction of this feature and its association, if any, with the two phases of buildings described above remains a mystery. Another cess-pit (13) contained 19th century material and is presumably associated with the cottage. The various other pits and depressions yielded several interesting assemblages of medieval pottery, including a large number of high quality imported wares.

Should the existing cottage ever be pulled (or fall!) down an excavation is likely to be rewarding since this would reveal the front ranges of the medieval buildings. The state of preservation of these is likely to be better than those to the north since the area has been protected by the cottage from destruction by recent gardening (the cottage foundations are minimal).

Acknowledgements

The Sussex Archaeological Field Unit is very grateful to the many people who gave help and advice during 1981. It would be impossible to list all their names, but we would like to acknowledge the following with thanks: Stephen Dunmore and Paul Gosling, F. G. Aldsworth, A. G. Woodcock, Dr and Mrs F. S. Kelleher, Lt.-Col. and Mrs M. B. Coleman, Mr and Mrs J. J. Coleman, the Reverend Davies, British Rail, Mr M. J. Langmead, James Hadfield, Mark Roberts, Arthur Sayers, Philippa Price.

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'The Secondary Products Revolution' and the Limitations of the Neolithic

by J. C. CHAPMAN

In 1979, a tribute to the late David Clarke appeared under the title 'Patterns in the Past'. A major contribution to this volume was an article entitled 'Plough and pastoralism: aspects of the secondary products revolution' (Sherratt, 1979a). In this chapter, Andrew Sherratt attempted to characterise what he regards as a crucial transformation of Old World agriculture. Settlement pattern and arable aspects of the main theme are treated in a separate article (Sherratt, 1980) whilst the model is summarised in one of the chapters of the *Cambridge Encyclopedia of Archaeology* (1979b). Together, these three papers make an important contribution to the history of Old World farming, such that an appraisal of their main ideas seems appropriate. This essay is intended to shed light on Sherratt's theoretical assumptions, methodological principles and dating arguments and concludes with an alternative view of the data.

The 'secondary products revolution'

The 'secondary products revolution of the Old World', to proclaim its full title, is perceived as a variety of similar, interlinked behavioural responses to a common problem, the pattern of population growth and territorial expansion of the Neolithic period and the long-term soil deterioration experienced in areas of Neolithic settlement (1979a: 263, 286). Five innovations dated in the Near East to the 4th millennium BC (henceforth MBC), combine to create the economic basis for a new settlement pattern — the light plough (or ard), wheeled transport, domesticated equids, a dairy industry and a textile industry based on wool. The basic materialist tenet of the theory is an expansion in the scale of animal keeping with important roles for oxen for plough-teams, milk cows, sheep reared for wool and equids for transport (1979a: 287). A parallel development is the intensification of agricultural production by the use of the plough which increased production and made economic the cultivation of a range of poorer-quality soils, the drier interfluves (1979a: 262). Similarly, in upland areas, where arable farming was not the optimal land use, sheep herding and transhumance were developed to boost the textile industry thereby introducing a new material into exchange cycles (1979a: 262, 289).

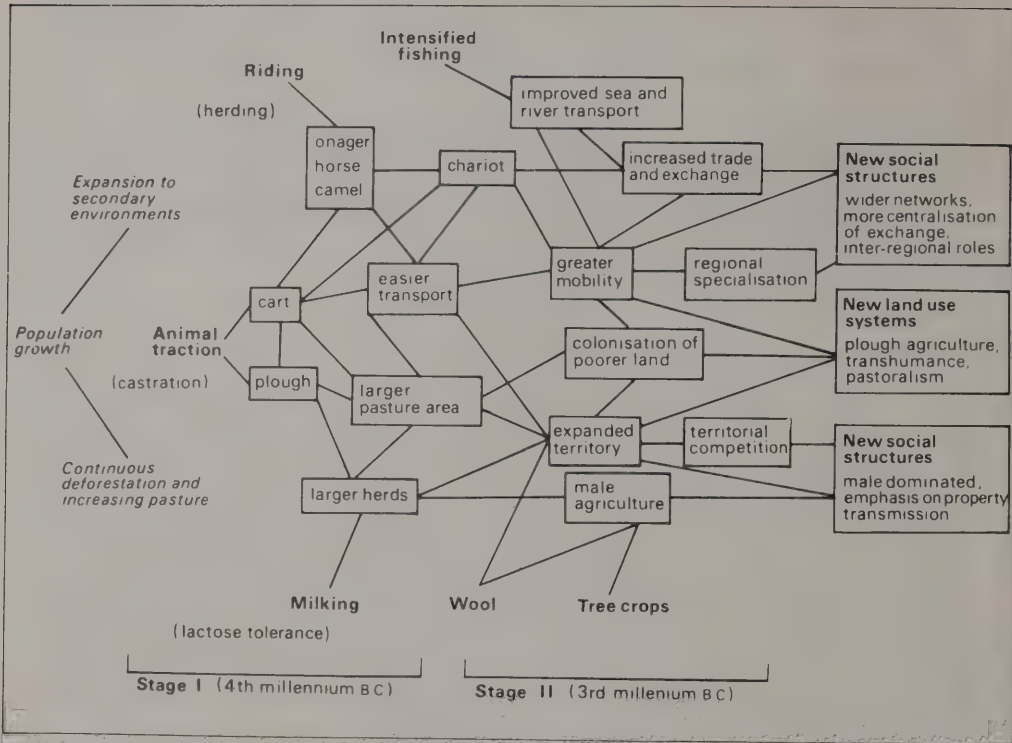


Fig. 1 Interaction of the components of the secondary products complex through time (From Sherratt, 1979a: Fig. 10.16).

Sherratt develops his theme by scrutinising the supposed results of the secondary products revolution (or 2PR) in two spheres of human development — settlement pattern and the economic system, and the socio-linguistic system. In the Near East, early farming sites are concentrated in restricted zones of local surface/ground water (1980). A later 6th Mbc development was the expansion to the deep brown soils of the interfluvies of north Assyria, based on early channel-irrigation (1980). The critical expansion to the major alluvial valleys, dated to the 5th or 4th MMBC, was based on more extensive irrigation, although the plough was an essential prerequisite of these developments; this expansion enabled a rapid population increase.

In temperate Europe, a strong contrast is made between early hoe cultivators and later plough agriculturalists. In the former case, farming sites exploited a narrow zone of maximum productivity, constituted by the fine-grained, water-retentive soils of the major river valleys. The economy is defined as a small-scale, locally intensive horticulture, in which ground-water is as important as rainfall (1980). Animal husbandry was restricted to small herds except in open country near the valleys, owing to low potential for the deforestation of the interfluvies. The later plough agriculturalists of the 3rd MBC

adopted a more extensive economy, clearing the forests to expand into these drier, less fertile interfluves. Although the plough made cultivation of the lighter, sandy or forest soils economic, nevertheless permanent cultivation was rare. Short periods of cultivation alternated with periods of long fallow, thus promoting a dispersed, homestead pattern of settlement. Later still, by the end of the 2nd MBC, deterioration of the light soils stimulated the development of the iron ploughshare for cultivation of the heavy clays and alluvial soils, the basis of Iron Age agriculture (1980).

In the socio-linguistic system, Sherratt views the spread of the Indo-European languages into Europe and the Semito-Hamitic languages into the Near East and Africa as closely linked with supposed pastoralist expansion by secondary products revolutionaries from the Caucasus and the Levant (1979a: 295–7). A concomitant change was a fundamental shift in social organisation from matrilineal descent with the dominant role of women in hoe horticulture to male-dominated subsistence activities, virilocal residence and patrilineal descent (1979: 297–8). These linguistic and social changes are viewed as setting the scene for the future course of European history.

If reliable, such a model is surely a significant step in understanding the development of Old World economies in the Neothermal period. To quote the author (1979a: 263): 'The secondary products revolution thus separates two stages in the development of Old World agriculture: an initial stage of hoe cultivation, whose technology and transportational systems were based on human muscle-power, and in which animals were kept purely for meat; and a second stage in which both plough agriculture and pastoralism can be recognised, with a technology using animal sources of energy. The secondary products revolution marked the birth of the kinds of society characteristic of modern Eurasia'.

General perspectives

If the term 'revolution' has any meaning for the prehistorian it implies the achievement of the potential for long-term cultural evolution through systemic change. It was in this sense that Childe coined the terms 'Neolithic Revolution' and 'Urban Revolution' almost 50 years ago (Childe, 1936). The 2PR shares with the former certain common characteristics typical of complex long-term behavioural change. Most importantly, the individual breakthroughs in the bundle of innovations characterising the Neolithic Revolution occur first at different times, often in different places and in sometimes functionally unrelated contexts (e.g. for the Neolithic, the development of grinding technology in the late Pleistocene and Early Holocene, or the occurrence of permanent villages without agriculture (Childe, 1936); the development of farming without the use of pottery (Childe, 1957)). Consequently, the full range of possible interactions between these innovations cannot occur until late in the sequence of change, thereby inhibiting development of the full potential of the changes (whose full potential is rarely initially realised anyway: cf. Ford, 1976, on domestication). In the absence of teleological

reasoning, 'revolutions' in the prehistoric sense can be nothing but cumulative and long-term. The paradox between 'revolutionary' potential and an actuality often synonymous with slow, gradual change may be noted as the primary difficulty in the very use of the term 'revolution'. A second question concerns the serious difficulties encountered in defining terms to which the word 'revolution' has been applied — most particularly 'Neolithic' and 'urbanism'. As Childe was quick to grasp, the behavioural variability subsumed beneath these terms requires the use of polythetic rather than exclusive classifications, yet there is an increasing trend towards attempting to identify and explain variability within terms assumed to indicate gross behavioural 'similarities'. If there is no longer much justification for retention of the term 'revolution', could a case be made out for using umbrella terms such as 'the Neolithic'?

Lest there be any confusion on this point, it should be stressed that Sherratt is the first to accept regional variations in rates of regional change and diffusion rates of the set of innovations in his cumulative 'revolution' (see below). Yet, *pari passu*, the greater the chronological and spatial variability in innovation/diffusion rates of any revolution, the more difficult the task of demonstrating the inter-relatedness of the so-called revolutionary 'complex'. There is a proportional relationship in terms of logic here — the tighter the time-space net cast around innovations, the greater the likelihood of an associated mode of behaviour; conversely, the wider the net, the more superficial the relationships between innovations is likely to be.

In the section that follows, it is important to bear in mind Sherratt's explanation of the occurrence of the 2PR in an evolutionary context, namely as an adaptive response to the problems of environmental deterioration caused by Neolithic climax settlement. It will be argued that the origins of the innovations packaged as the 2PR are in fact even more diffuse in time and space than Sherratt would have us believe and consequently, that the onus of proof of genuine associations between these innovations and Neolithic over-exploitation rests more heavily on the author's evidence that it can support. As an alternative explanation to this theory, it is proposed that, far from being a response to the decline of post-climax settlement, many secondary products were utilised in Neolithic climax societies *themselves* and may have led to environmental over-exploitation in the late Altithermal period.

Methodology

It is widely agreed amongst scientists that the measure of whether a hypothesis is scientific or not is the likelihood of the hypothesis being falsified if it is in fact false (Popper, 1959; Medawar, 1967). By using this criterion, it would appear that Sherratt's hypothesis is indeed scientific, since the logic of refutation can be applied in a straightforward way. Put formally, the hypothesis is that the group of inter-related innovations comprising the 'secondary products revolution' arose in the Near East (4th MBC) and Europe (3rd MBC) in response to the environmental over-exploitation

ultimately related to the cycle of climax Neolithic settlement. This hypothesis can be falsified if any of the following propositions are documented: (a) the innovations are not functionally related; (b) some or all of the innovations occur during or prior to Neolithic climax settlement; (c) factors other than the environmental over-exploitation of Neolithic climax societies were responsible for the innovations of the 2PR.

It is unfortunate, given the scientific nature of his hypothesis, that Sherratt attempts neither the derivation of any stringent tests for his hypothesis nor its falsification in any meaningful way. Indeed, as will be shown in comments on the key section on 'Dating Evidence' (1979a: 253–85), Sherratt misses an obvious opportunity to modify the original hypothesis, thus paving the way for alternative models.

This epistemological stance leads Sherratt into two hazardous procedures – the derivation of conclusions from a limited or biased data set and the uncritical acceptance of his own speculation. Each of these procedures will be discussed in turn.

Before discussion of these methodological points, it is instructive to consider factual matters related to the chronology of the innovations discussed above.

The omission or rejection of data incompatible with favoured hypotheses is almost as common in archaeology as are archaeologists (this author included). Nevertheless, a surprisingly lengthy list of discrepancies is quoted here to set the 2PR in some perspective. These will be considered by topic.

It should be stressed that Sherratt himself makes giant strides towards admission of non-contemporaneity, despite classification through the inconsistent and confusing alternation of the labels 'BC' and 'bc' applied to radiocarbon dates and, more often, millennia. In the case of plough agriculture, Sherratt's views change from the 4th MBC in the Near East (1979a: 262) to possibly in the 5th Mbc in N. Assyria, i.e. the 6th MBC (1980: 319). Whilst it is doubtful whether the evolution of lactose-tolerance and hence milking can ever be closely dated (see below), Sherratt reluctantly accepts the independence, if near-contemporaneity, of five episodes of equid domestication (1979a: 275); not all in the 4th MBC as he would have it, but on his own evidence in the 5th MBC (Balkans–horse: p. 273) probably the 4th MBC (Assyria–Khuzestan–onager: p. 273), and the 3rd MBC (Palestine–ass: p. 274, and camelids: p. 275). It would not be unfair to compare this juggling with millennia with an illusionist's tricks; indeed attentive readers could be excused for thinking they were passing in and out of a time-warp.

The cart

Since clay model wheels are mentioned in Near Eastern Early Dynastic contexts (1979a: 263), it is proper to recall similar 'model wheels' from Balkan Gumelnița and Petrești contexts, dating to the first half of the 4th Mbc (mid-5th MBC) (Piggott, 1968: Fig. 3; Mateesco, 1975: 17). Combined with the evidence for use of the plough in the Balkan Neolithic (see below), the 'model wheels' may indicate an initial understanding of rotary motion, which may or may not have led to the development of carts.

The plough

The fragility of the inductive approach — that one or two fragments of new evidence can cast doubt on a conclusion based on far more data — is underscored in the case of Sherratt's evidence for ploughs. Recent confirmation of ploughing in the late 5th Mbc (early 5th MBC) in the Near East is provided by the discovery of plough-marks in Susa A contexts (Wright *et al.*, 1980: 275). A recent evaluation of the hypothesis of 5th–4th MMbc ploughing in the Balken Neolithic concluded a strong likelihood on the basis of territorial evidence supported by certain technological data (Chapman, in press). This conclusion is now reinforced by the discovery for the first time in Central Europe of ploughmarks beneath an Eneolithic (late 4th Mbc = mid-4th MBC) site in Czechoslovakia (Pleinerova, 1981). In addition, the plough-marks found beneath the TRB barrow at Sarnowo dated by Sherratt to 2700 bc (3400 BC) can be dated anywhere between 3500 and 2800 bc (4500–3400 BC), contemporary with the Czech example.

Finally, as if to emphasise the availability of precursors of or parallel developments to central and south-east European traction ploughs, the Ellerbek site of Satrup Moor yielded several almost complete wooden tools, the longest interpreted as a rope-traction ard (Steensberg, 1973). Essentially an elongated spade designed for human traction, this tool indicates the progress made in agriculture by a group still partly dependent on hunting and gathering and indicates the potential for innovations in northern Europe. The find is dated *c.* 3500 bc (late 5th MBC), i.e. contemporary with or earlier than the Sarnowo plough-marks.

The significance of these finds is that they effectively extend the chronological pattern of innovation fundamental to the 2PR to the 5th MBC in the Near East and Europe. At this stage it is difficult to falsify the hypothesis that plough agriculture developed as early in south-east Europe as in the Near East — in the late 5th–early 4th MMbc (early 5th MBC), one or two millennia earlier than envisaged in the 2PR hypothesis.

Equids

There is little to add to Sherratt's implicit acceptance of a spread of two millennia for the domestication of the full range of equids. Two minor points concern the evidence of Bökönyi (1974: 238, 240) relating to limited horse domestication in the Early and Middle Copper Age of the Hungarian Plain, dated *c.* 3400–3000 bc (4000–3600 BC), and the tentative claim for 5th Mbc (6th MBC) camelid domestication from southern Anatolia (J. Mellaart, pers. comm).

Milk

The dating of the evolution of lactose tolerance is discussed below. At this juncture, a comment is offered on changes in pot shapes in relation to their function/contents. If any certain conclusion can be reached (and this appears extremely doubtful), certain

widespread trends should be recalled in the pottery shapes of the Balkan late Neolithic (Chalcolithic). In the bowl and dish repertoires of the late Vinča, Gumelnița, Petrești and Cucuteni cultures, there is a marked increase in the production of wider, shallow vessels. This trend is not inconsistent with increased use of dairy products, especially the production of cheese and curds, and occurs simultaneously with the development of handled vessels (a defining characteristic of the late Vinča assemblage) and specialisation in storage jar and amphora forms (Chapman, 1981). Perhaps the conclusion is that one should not be blinded by speculative dating of genetic changes to the possibilities of alternative economic developments.

Wool

Sherratt's discussion of the dating of woollen textiles with reference to Çatal Hüyük reveals, in microcosm, the weakness of inductive logic and his own approach. Sherratt readily quotes Ryder's (1969) rejection of the identification of wool from carbonised organic remains from Çatal Hüyük in support of his hypothesis, but completely ignores Helbaek's (1963) equally vigorous denial of the presence of flax in botanical samples, which runs counter to the 2PR thesis. Here over-simplification can never be a substitute for restrained evaluation of a divergence of views between two distinguished specialists over the Çatal textile evidence.

Perhaps a more serious omission from discussion of weaving technology (1979a: 298) is the south-east European later Neolithic data, much of which occurs in Vinča contexts (Chapman, 1981). At least six variants on fired clay loom-pieces are known, starting in the late 5th Mbc (mid-5th MBC). At least two local areas of innovation have been identified – an upland zone in west Rumania (probably using sheep's wool) and a lowland zone in the middle Morava valley (with appropriate hydrological conditions for the retting of flax but with the option of using sheep's wool). This south Balkan development is contemporary with the production of Tisa pottery, whose characteristic complex rectilinear motifs have been likened to textiles (e.g. Banner, 1971). As before, this evidence for specialisation on textile production dates far earlier than the 3rd MBC in Europe.

Tree crops

Although Sherratt quotes with apparent approval the work on Bronze Age fruit domestication by Zohary and Spiegel-Roy (1975), he omits the earlier study on Neolithic botanical remains in south-west Russia (Janouchevitch and Markevitch, 1971). In this work, the Russian botanists list the discovery of a wide range of fruit remains, including the apricot, which is found with the Mirabelle plum and intermediate remains suggesting possible hybridisation. These precocious developments are, as with ploughing and horse domestication, dated to the Tripolje B phase (c. 3800–3400 bc; 4400–4000 BC).

Transhumance

There are few more controversial statements in Sherratt's work than that mountain pastoralism in the fold mountains of southern Europe is a 3rd MBC creation (1979a: 289). In addition to other areas with evidence for Neolithic transhumance (Dalmatia: Chapman, 1982; Switzerland: Higham, 1968), the data from central Italy have been clearly summarised (Barker, 1975). There is abundant evidence from the 5th Mbc onwards that lowland sites in the Marche, the Abruzzi and the west coast were symbiotically linked with Apennine upland summer pasture sites. Similarly, in temperate Europe, there is evidence from First Temperate Neolithic, Vinča, Petrești, Tisa, Tisapolgár and Gumelnița contexts of the exploitation of upland grazing through summer transhumance (Chapman, 1981; Bognar-Kutzian, 1972; Dennell, 1978; cf. Sherratt, 1972: Fig. 12.16). The importance of upland grazing had been recognised and exploited since the Palaeolithic; there was no reason for this long-term adaptation to cease before the Bronze Age.

Mining and quarrying

Sherratt's claim (1979a: 292) for the appearance at a similar date (the 3rd MBC) from Britain to Russia of mines and quarries for axe material for forest clearance is surely a case of the mistaken identify of phantasy for fact. Whilst early working at several flint mines and stone quarries date to the 3rd Mbc, at least six major sites are 4th Mbc (late 5th or early 4th MMBC), one of which, the flint mine of Mauer in Austria, has a Lengyel attribution (cf. Sussex mines, Britain: 3300 bc onwards; Spiennes, Belgium: 3470 ± 75 bc; Mesvin, Belgium: 3390 ± 150 bc; Ryckholt, Belgium: 3150 ± 60 bc; Krzemionki, Poland: 3375 ± 90 bc and 3096 ± 102 bc). If the handful of early copper mines in Europe (Rudna Glava, Ai Bunar, Huelva, etc.) is taken into consideration, it becomes clear that European mining is largely a 4th Mbc phenomenon (5th MBC) (Chapman, 1981; Shepherd, 1980), over 1500 rc years earlier than Sherratt's postulated date.

A related point concerns the claim that the Linearbandkeramik settlers, lacking wide-bladed axe forms, were unable to clear the mixed oak forest of the interfluves (1979a: 292; 1980a: 316). Since the term 'wide-bladed' has not been defined, it is difficult to know whether Sherratt considers the shoe-last axes — some of the heaviest axes of the European Neolithic — insufficient for woodwork. In any case, rejection of slash-and-burn (see below) is surely not synonymous with rejection of deforestation by fire-setting.

In summary, such are the chronological vagaries in the appearance of these innovations that a dispassionate reader may feel the only possible conclusion is the falsification of the original hypothesis. However, if this conclusion is not reached on chronological evidence alone, there are equally serious methodological objections and to these we now return.

Bias

Far from being limited to the 2PR model, the derivation of conclusions from a biased or limited data set is ubiquitous in archaeology. Recognition of this fact has led to a greater care in establishing the sampling framework for research and more caution in the interpretation of results. Sherratt is certainly aware of this general problem: 'the archaeological evidence for this range of secondary uses and products of domesticated animals is naturally very varied and of uneven value' (1979a: 262). Nevertheless, there are specific disjunctions in the data set which inhibit comparability. The most obvious asymmetry is between data from Europe, entirely prehistoric in nature, and data from the Near East, with its literary and iconographic records dating from c. 3200 bc (c. 3700 BC).

It is not without significance that the earliest indications quoted by Sherratt of four of the five traits of the 2PR are provided by pictorial, iconographic or literary evidence in the Near East (Sumerian pictograms for the cart (p. 263) and the plough (p. 266); iconographic evidence for milking (p. 280) and pictorial and literary sources for wool (p. 282)). In the absence of earlier literary records, such forms of evidence can provide only *tempora ante quos* for these developments. For instance, in the case of the cart and the plough, the first pictorial records precede the earliest artefactual evidence by several centuries, yet the Susa A plough-marks precede the former by over 500 years. It can also be assumed that, since there were over 30 Uruk pictographs representing what in some cases were different breeds of sheep (1979a: 232), specialisation in caprine production predates the pictorial record.

Similar problems of preservation occur in temperate Europe. Ploughs and carts are rarely preserved except in waterlogged deposits; until recent years, plough-marks have tended to be discovered only in areas where they are preserved under sizeable field monuments and vehicle burials are rare outside areas where pit burial often under barrows is the norm. The net effect of such bias is discrimination against south-east and central Europe in favour of north-west Europe, with its tradition of earthworks and megaliths and a far higher rate of peat growth, and south Russia, with its barrow-burials.

Such inherent biases in the data set make the dating of the 2PR to the 4th MBC in Near East and the 3rd MBC in Europe more readily comprehensible, although there is additional unbiased evidence for this viewpoint (e.g. the Balkan Copper Age carts and bovid burials; the Alpine record of textile production, etc.). The point is that future research should be concentrated in areas where there is a preservational bias as a corrective to the formulation of ill-balanced conclusions on the basis of uneven data.

Speculation

Potentially more serious than the acceptance of biased data at face value is the uncritical acceptance of speculation. Whilst Sherratt is prepared to admit that 'the arguments concerning early cultivation systems are often speculative' (1980: 313), it is readily appreciated that some of the basic data in support of the 2RP are based on

unverified/unverifiable assumptions. Three sets of assumptions merit further analysis – lactose tolerance, the ceramic evidence for a dairy industry, and the interpretation of faunal spectra.

In assessing the evidence for dairy industries, Sherratt concludes that adult milk-drinking is a late and restricted feature of the human diet, since lactose tolerance is the result of a relatively evolutionary episode (1979a: 276–80). The source of these views is the spatial distribution of milking and non-milking areas in Eurasia, whence it is concluded that the existence of non-milking enclaves suggests a recent expansion of the practice and that the overall distribution suggests a ‘mid-Holocene origin for this practice, somewhere in the central area . . . the mountains of the Near East’ (1979a: 274). On the basis of this conclusion, Sherratt suggests ‘it seems likely that milking and milk-consumption are another aspect of the 2PR’, later that ‘milking was probably a pre-condition for these developments’ (p. 277).

Despite the fascinating interplay of genetic and cultural factors in this discussion of the development of milking, the reader has no solid biological evidence on whether a mid-Holocene date is the most likely date or, conversely, why milking was not related to the obvious alternative – the spread of the Neolithic way of life.

The question of lactose tolerance leads naturally to the next question – how does one define and recognise a ‘dairy industry’ in the prehistoric period? The new pottery shapes of the Copper Age/Early Bronze Age are clearly a functional response to new drinking habits; the problem is which habit or habits and are they related? The alternatives to milk for many of shapes is some form of alcoholic beverage, whether beer, mead or wine. It should not be overlooked that the distribution of the wild grape stretches up the Danube into the Pannonian basin (Renfrew, 1973; Zohary and Spiegel-Roy, 1975), so that the Baden associations of cart burial, jug, dipper and bowl (p. 282) could as plausibly represent a genuine problem of alcoholism on the road! It is perhaps surprising, given his enthusiasm for dairy products, that Sherratt has not suggested the likelihood of milk Beakers in the 3rd MBC, but that is another story.

In his brief characterisation of the economics of the 2PR, Sherratt accepts that the ‘handful of systematic investigations needs to be multiplied many times before comparative patterns (of faunal spectra) can be defined’ (1979a: 285). Nevertheless, the temptation to assume the kill-off patterns typical for the 2PR is too strong, with the resultant pure speculation that domestic animals before the 4th–3rd MMBC were exploited purely for their meat and that the spread of the 2PR brought about major changes in animal husbandry. Sherratt is surely correct to focus attention on mortality structures correlated with different uses, but the field is too young for over-hasty conclusions. It would seem that many a revolutionary road is paved with good intentions.

Theory

After evaluation of the dating evidence, Sherratt advances a general model to explain socio-economic patterns before and after the 2PR (1979a: 285–99). At best,

the model is a virtuoso piece of the highest creativity, at worst a flamboyant exercise in otiose speculation. It posits complex behavioural changes to explain settlement pattern and socio-linguistic changes in Eurasia. At this juncture, it is wise to recall the general problems of testing explanations of this type which rely on multiple variables (Wright, 1976). Since neither experiment nor statistical control of variables is feasible, what alternatives remain? In the case of the 2PR, Sherratt is content to present a number of variables in diagrammatic form (1979a: Fig. 10.16 here = Fig. 1). It would appear that Sherratt's model is a type of greatly over-simplified formulation rather than an actual phenomenon. Yet although interaction of (some of) these variables is implied, it is unlikely that all the variables in any given system are included and the systematic relationships of these variables included are neither specified nor obvious. Some of the 'interactions' denoted in Sherratt's (1979a) Fig. 10.16 (here reproduced as Fig. 1) are decidedly curious. There is no clear link between 'greater mobility' and 'regional specialisation', or 'larger herds' and 'male agriculture'. Conversely, well-documented relationships between regional specialisation and increased trade and exchange and colonisation of poorer land are overlooked. Indeed, it is hard to describe the model as more than an assertion of potential correlations supported by selected evidence, which may indicate relationships ultimately requiring explanation. On the basis of informal logic alone, the 2PR hypothesis is threatened by Sherratt's failure to systematise the relationships between variables; the difficulty of the task is symbolic of the problems which beset the supporters of systems theory in archaeology (Clarke, 1968; Flannery, 1972).

Having examined the 2PR in the light of general principles of model-building, it is apposite to turn to the specific settlement pattern changes envisaged in Europe.

The self-confessed speculation (1980: 313) of the settlement pattern reconstructions cry out for critical comment, in view of their reliance on a serious misunderstanding of environmental parameters.

I have already summarised the contrast made by Sherratt between the agricultural systems of early and late cultivators in Europe – the use of fine-grained water-retentive river valley soils by the former, of altitudinally higher interfluvial soils by the latter. Two questions arise from this grossly oversimplified comparison, (a) the nature of the interfluves in different parts of Europe, and (b) the location of the early agriculturalists' cultivated areas.

A brief glance at any geomorphological or soil map of Europe will indicate the considerable variety in interfluvial substrate and soils, both in terms of different soils on the same substrate and its converse. To characterise interfluves in central and south-east Europe as lighter and less fertile than alluvial soils given the appropriate technologies is indefensible. The basic confusion appears to lie in the nature of loess. The loessic substrate itself is, of course, dry, well-drained and light; however, soils developing from the loess substrate can be heavy yet fertile chernozoms, medium-grade moderately fertile soils such as brownearths or light sandy diluvial soils. Hence, in south-east Europe, most interfluves in the lower Danube valley are chernozoms and brownearths on loess, whilst in the Morava valley, brownearths and smonicas prevail. Similar variation occurs

in central Europe; e.g. the variation in soils in little Poland (Kruk, 1973) and central Germany (Sielmann, 1971). It is particularly common in the north European plain that interfluvial tend to be sandier and lighter than the riverine clays; it is revealing that the example Sherratt quotes for plough-cultivation of sandy interfluvial is the Bronze Age in Jutland! (1979a: 293). (It is revealing, however, that the soils cultivated by the 3rd MBC light ard in northern Europe at sites such as Aldrupsgaarde and Aptrup are heavy clay deposits (Wailles, 1970: 287).) By definition, interfluvial soils are drier than flood-plain soils yet in all other parts of characterisation, Sherratt's model is misleading.

The question of the location of early farmers' cultivated plots follows directly from this confusion. Sherratt appears undecided whether the gardens (or fields) of the LBK are on 'hydromorphic soils' in the flood-plain (1979a: 315) or on the loess of the river terrace (1979b). At one moment, Kruk's (1973) conclusion of flood-plain horticulture in little Poland is quoted with approval (1979a: 316); in the next, we are told that in the 4th MBC, the loess became less tractable 'as a result of cultivation' (1979b: 146). Hence the argument becomes 'which resource has the potential for long-term fertility – loess-derived soils or river alluvium? Whilst the annual silts laid down by *major* rivers in central Europe could satisfy intensive horticultural demands, the majority of LBK sites in the Aldenhovener Platte (Lüning, 1976) and many other areas (e.g. C. Germany; Sielmann, 1971; Milisauskas, 1977; U. Rhine: Balkwill, 1976) lie near small streams with far lower capacities for alluvial deposition. So the 'hydromorphic' soils of central Europe can hardly be compared with those of the Alföld plain in Hungary, where many LBK sites lie on levees created through massive spring flooding and silt deposition (Kosse, 1979). Hence it seems logical that in loessland areas of high depositional activity, flood plain horticulture was practised in the LBK period, whilst in areas with less active deposition, LBK farmers exploited the loess-based soils of the river terraces. If correct, this hypothesis reduces the 'gap' between agricultural systems of early and late farmers upon which Sherratt places so much reliance. The hypothesis can be tested by defining the area of alluvial soil available to LBK farmers for a given range of site populations.

A further point concerns the nature of soils exploited in Mediterranean and temperate European environments. At first sight, Sherratt's notion of ground-water and flood-plain horticulture in the early farming period in Greece, the Balkans and central Europe (1980) would appear to conflict with Nandris' (1970) distinction between the preference for water-retentive soils in the Mediterranean zone and the selection of well-drained soils in the temperate area. I suspect that the apparent divergence of views relates to the different scale at which the problem is viewed. Whilst there is little doubt that most FTN settlement in the Balkans was concentrated near the major rivers, locations of low water-table were selected within that narrow zone (Nandris, 1970: Maps 1 and 2). Perhaps a significant difference between farming potential for the FTN and LBK settlers was the greater depositional activity of most of the major Balkan river systems in comparison with the lower rate of alluvial deposition in the loessland basins. It may be possible ultimately to relate such variation to differences in population densities/sizes in long-term settlements such as Bulgarian tells and LBK homesteads.

The second major criticism of Sherratt's settlement pattern model is the almost total neglect of regional later Neolithic developments (in central Europe, the Rössen–Michelsberg–TRB groups: in the Balkans, the far longer-lasting Dark Burnished ware groups and the 4th Mbc 'climax societies'). There is clear evidence of later Neolithic settlement on the interfluvies of many major rivers in the Balkans (Chapman, 1981), not to mention little Poland (Kruk, 1973) and the Aldenhovener Platte (Lüning, 1976). If indeed the opposite were assumed and the cumulative soil resources of the interfluvies built up throughout the Altithermal were exploited for the first time in the 3rd MBC, the expected pattern would be a far more stable, more intensive settlement than occurs in the 'missing millennium' of south European prehistory (3500–2600 bc in Greece, 3000–2300 bc in the Balkans, 2900–2300 bc in Italy). An alternative hypothesis emphasises the role of the plough in extending the subsistence base of large later Neolithic population foci (Chapman, *in press*). Whilst the central effect of the plough was to reduce labour input, the option of expanding the cultivated area for the same effort was available in case of population increase. The combination of increased pasture for a growing animal component in subsistence economy and the larger areas of cultivated arable necessitated more widespread use of the interfluvies, with consequent deforestation and soil erosion in some areas (e.g. southern Bulgaria; Dennell and Webley, 1975; the Capitanata: Jarman and Webley, 1975). In other areas, expanding requirements for arable and grazing land stimulated the exploitation of complementary wild resources (e.g. Tisa-Herpály sites in eastern Hungary: Bökönyi, 1974; the Gumelnița site of Căscioarele, with a faunal spectrum dominated by 80% wild animals: Dumitrescu, 1965).

Hence the decline of Neolithic climax societies was closely related to the interactions of plough agriculture and higher population size/density over a millennium of intensified settlement. The low-intensity, extended settlement patterns of the 3rd Mbc are a response to soil deterioration in valleys and interfluvies alike, foreshadowed in some areas of the Balkans by extensive Middle Copper Age settlement, and not merely a consequence of novel late Copper Age pastoral practices.

Summary

Sanders once stated: 'Flexible methodologies provide the great theories, not the rigorous methodologies' (1976: 275). Since there is some justification for the claim that Sherratt's methodology is indeed flexible, can the 2PR be considered one of the great theories?

The most favourable perspective (i.e. retrospective) on the 2PR is gained from the vantage-point of the Bronze Age of the Near East and Europe. The plough and the cart, the equids, wool and milk may indeed have been fundamental features of Bronze Age life, each conceivably as important as bronze itself. And in so far as Bronze Age economies provided a solid evolutionary springboard for later developments in Eurasia,

it is a truism to say that secondary products would have played a significant role in those developments. But there is substantial evidence that those secondary product innovations which are datable in the archaeological record (and I exclude milking and wool) are not only part of the Neolithic adaptations of the 5th–4th MMbc, but may even have contributed to the deterioration of the landscape encountered by Bronze Age populations. And even if the full range of these products was first experienced in the 3rd MBC, the systematic specification and explanation of such hypothetical relationships is still far from our understanding.

We are thus entitled to conclude that whilst the use of secondary products of animal husbandry gradually revolutionised the economic basis of European and Asiatic populations, the timespan of this transformation has been grossly underestimated by the author. Whilst Sherratt's view of the end-products of the 2PR may be accurate, his view of the routes taken by the revolutionaries is deficient and often misleading. Whilst parts of the hypothesis reveal the power and penetration of the materialist paradigm, this preliminary formulation has far to go before it can be hailed as one of archaeology's great theories.

Abstract

This article reviews Andrew Sherratt's model of the transformation of Bronze Age Eurasian economies termed the 'Secondary Products Revolution of the Old World' (2PR). Sherratt's interpretation of the bundle of innovations comprising the 2PR – the ard, wheeled transport, domesticated equids, a dairy industry and a woollen textile industry – is criticised on the basis of dating divergences, methodological bias and speculative reasoning. Far from representing an adaptive response to problems of environmental deterioration related to Neolithic climax settlement, the 2PR is in fact spread over at least two millennia in both Europe and the Near East. According to an alternative economic reconstruction, European developments such as the ard, woollen textiles and dairy products were integral to Neolithic climax settlement and, as a result, contributed to lower environmental potential at the start of the Bronze Age.

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Computer Recording of Archaeological Excavations

Introduction

by IAN GRAHAM

This group of four papers describes research into the computer recording of excavations currently being carried out by University of London Institute of Archaeology students or in collaboration with the Institute.

The first microprocessor chips were produced by Intel in 1972, with other companies soon following. By about 1975 the chips were making their appearance in microcomputer systems. At first these were in the form of 'intelligent terminals', which were in fact small self-contained computers, complete with mass data storage on cassette tape, but intended for use as terminals to other, larger, machines. It was clear then that such machines might represent a cost-effective means of on-site data recording for archaeologists.

At that time a few experiments had been made in the computer recording and analysis of site material, either using on-site terminals or coding sheets and punched cards (Newell and Vroomans, 1972; Buckland, 1973). In a paper at the 1976 Computer Applications in Archaeology Conference (Graham, 1976), I described that data capture techniques that had so far been tried, and suggested that the new intelligent terminals might have some significant advantages.

The main tests of any data recording method are those of speed, error rate and cost, plus the ability to adjust rapidly to changing archaeological circumstances. The problem with punched card input was that it was slow, inaccurate and expensive. Interactive terminals on-site provided more speed and lower error rates through the use of interactive data checking, but telephone line charges to the remote computer could be insupportable.

This has changed little in the last six years. In fact the cost of data punching and telephone lines has increased, and delays in the provision of telephone lines are still too long in many areas to be of any use to the rescue excavator. However, the new telephone services, such as British Telecom's Packed Switched service, will decrease the cost and increase the speed of long distance data transmission. But both the decline in the cost of microcomputer equipment and the increase in its power have been dramatic. In 1976 the Institute received a grant from the British Library for the purchase of an 'intelligent

terminal' to investigate its use for site recording. At the time of the grant application it was intended to purchase a machine based on a slow version of the 8008 microprocessor, 16 kbytes of memory, and cassette tape. By the time the grant was announced, just a few months later, it was possible for us to purchase a much more powerful Zilog software development system. This had a Z80 microprocessor, with 32 kbytes of memory, and two 256 kbyte, 8 inch floppy disk drives. The machine cost about £5,500 in 1976, which was before the American explosion in personal computing reached England. A machine of similar specification would now cost about £2,000. This must be set against inflation of more than 100% since 1976. Of course, today no one would buy a machine with only 32 kbytes of memory — 64 kbytes has become almost standard, and the capacity of 8 inch floppy disks has been increased to over 1 Mbyte. The purchasing power of £5,500 in 1976 would now enable you to buy at least three more powerful machines.

The Zilog development system was used to write the initial data entry and retrieval programs that were demonstrated at the 1977 and 1978 Computer Applications Conferences, and described in detail in the British Library project report (Graham, 1980). At about this same time the two other early computer recording systems were being set up, at Maxey by Francis Pryor (Pryor, 1980; Crowther and Booth 1981), and in the Department of Ancient Monuments and Historic Buildings of the Department of the Environment (Jefferies, 1977). The Maxey system was written in the BASIC language, and ran on an Apple microcomputer. The Directorate's system was initially set up on a large timesharing machine in America, and accessed by transatlantic cable. Later, however, it was re-written in BASIC to run on a Research Machines 380Z microcomputer (Benson and Jefferies, 1980).

At that time very little software was available commercially for microcomputers. Our first machine had only a primitive operating system, an assembler and a rather inefficient BASIC interpreter. Thus the first programs were written in assembly language, which allowed quite a complex data entry program to give a very rapid response. This was, of course, at the cost of much more programming effort than would have been needed if we could have written in a proper high level language.

At the Institute we have continued the development of the data entry and retrieval programs, as is described in the accompanying paper by Jonathan Moffett. We have benefited greatly from our collaboration with the Mucking Excavation and Post-excavation, which has provided an almost inexhaustable supply of things to record.

The present

The picture now is very different from the late 1970s. The rapid rise in personal computing has not only driven hardware prices down, but has also resulted in an enormous increase in the amount of software that is available.

A standard operating system, CP/M, has emerged, which runs on microcomputers based on the most common microprocessor chips, the 8080, 8085 and Z80. This has opened up a very large market for software authors, which also means strong

competition and lower prices, much lower than those traditionally associated with mini- or large computer software.

Almost all the languages now found on large-scale computers are now available in full-specification, well-debugged compilers or interpreters. All the applications software recently written at the Institute has been in Pascal: we still avoid BASIC as a language that we think ill-suited to large programming projects. As well as languages, many packages are now available that can save work in software development, as well as making the microcomputer more generally useful for such tasks as word processing and accounting. The most important packages for archaeologists are probably the Database Management Systems, such as MDBS and dBASE. These packages allow the development of programs to control archaeological databases much more quickly than would be possible using conventional file manipulation. DBMS packages can sometimes be rather profligate with disk space, but this factor is becoming less important as the cost of 'hard' disks reduces. Our most recent development system has 20 Mbytes of disk space, which is probably sufficient for all except the largest site.

Purchasers of packaged software should, however, be aware of its limitations. We would still be unable to purchase a replacement for our data entry program, which would be as easy to use and provide the kind of data checking that we require. The possible problems with packages may also be illustrated by comparing the two DBMS packages mentioned above, MDBS and dBASE, which are quite different in concept and in the nature of the problems they produce for both programmer and user.

MDBS is a very complex system, which can only be accessed through a host language, such as Pascal or BASIC. Thus, even to start using the system one must be a programmer. However, once the programming is mastered it is very powerful, with few limitations on the size of the database or the type of data that can be manipulated. The limitations that do arise come mainly from the limited memory of an 8-bit microprocessor, not from the package itself. It provides password protections for data files, and a facility for recovering data after machine faults or operator errors. Versions of MDBS are available for non-CP/M micro and minicomputers, so an application can be moved to a larger machine if necessary.

dBASE, on the other hand, is a completely self-contained program, needing no programming skill to use. It is thus possible to create a small database application very quickly. The package has the disadvantage that it cannot be expanded by writing in a host language. If the data entry routine provided by dBASE is not satisfactory there is no way to change it. There is no way of protecting against system failures except by keeping a copy of the entire database, and there are limitations on the size and number of records that make it unsuitable for large problems. But it still remains a very good package for small databases which need to be searched in simple ways.

The future

The future of microcomputers in archaeology looks bright. There are at least three software systems available that would be considered for recording and analysing a site.

It is very unlikely that any archaeologist will need to write such a system again, though of course he or she may want to. The use of DBMS packages is making it easier to write packages to deal with other archaeological problems, such as sites and monuments records. We are at present working on a sites and monuments program, based on MDBS, with the co-operation of the Bedfordshire County Archaeologist.

The most important hardware development in the near future will be the adoption of 16-bit microprocessors. Although these will give an increase in speed, a much more important effect will be the tremendous increase in system memory that is possible with microprocessors such as the 68000. This chip has a 16-bit data path, but operates on 32-bit numbers internally, and uses 24-bit addresses. This allows us to increase the addresses space from 64 kbytes with the present 16-bit address, 8-bit data microprocessors, up to 16 Mbytes with the 68000. This can cut down on disk access time, and have a much greater effect on computing throughput than a comparison of the speed of 8-and 16-bit micros would lead us to suppose. The project by the MDA to move their GOS package on to a 68000 microcomputer is a good example of the kind of software that will be available on personal machines within the next few years. More and more the microcomputer will be able to handle the complete computing needs of archaeological interpretation, not just the data capture and simple retrieval that we have at present.

Notes

MDBS is produced by MDBS Inc, dBASE by Ashton Tate Inc, and both are available from British software suppliers.

This work was originally supported by the British Library Research and Development Department.

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The Generation of Archaeological Excavation Computer Databases

by J. C. MOFFETT

Introduction

Archaeology is a discipline that produces considerable quantities of artefact and feature data often in only a few weeks of excavation. The examination and collation of these data to produce the final report often takes considerably longer. It is for this reason that archaeologists are now turning to computers to store and manage their data, after a period when the purpose of computers was mainly confined to statistical analysis. The major reason for this change in use is the advent of relatively cheap hardware, so that it is now feasible for archaeological units to buy microcomputers. Although inexpensive, these machines are usually Z80 microprocessor-based systems with one or two floppy disk drives and are not really capable of storing and handling large quantities of data efficiently. Computers with a greater storage capacity, for instance microcomputers or mini-computers with hard discs, are required for the long-term storage of data.

There are various methods of controlling large quantities of data, but the use of Database Management Systems (DBMS), particularly on mini- and microcomputers is a comparatively new area of investigation. This paper describes the background to research which is continuing at the Institute of Archaeology into the application of DBMS for archaeological excavation data.

The problem

The problem is really one of how the archaeological data recorded by a survey or excavation, are to be transferred to, and then stored efficiently and effectively on, a computer so that they can be used by the archaeologist to understand the site, while at the same time allowing the data to be safely and permanently stored. There have been a number of possible solutions proposed to this problem, although these have tended to concentrate on aspects other than permanent storage of the data. Further, these solutions

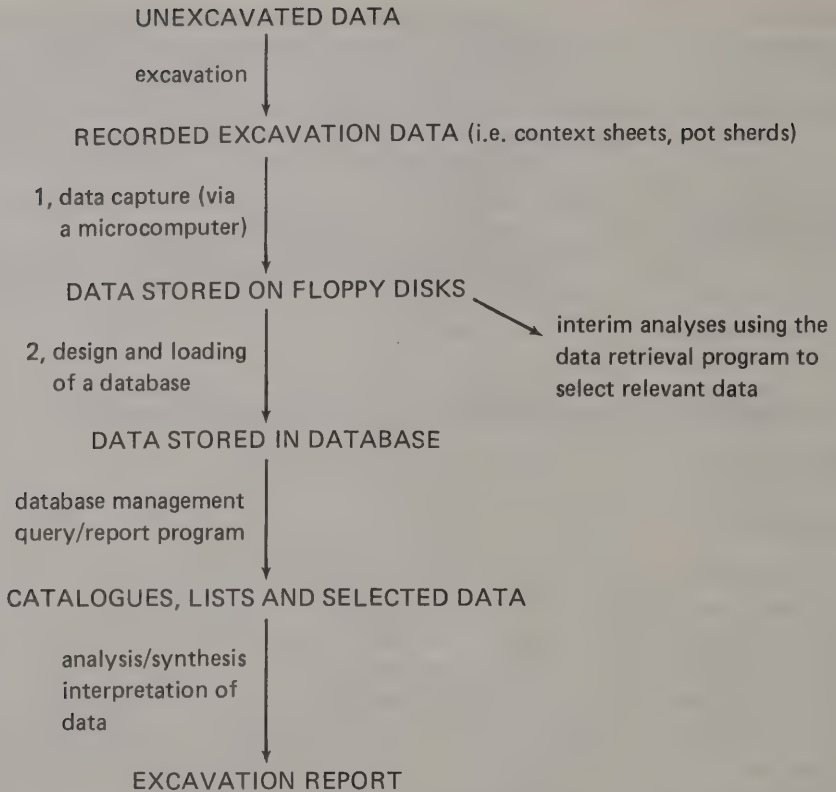


Fig. 1 Flow diagram of the archaeological excavation process. The different types (upper case) of data which result from each stage are linked by arrows and an explanation (lower case) of the process involved. The two stages described in this paper are labelled 1 and 2.

have tended to be limited to data which are relevant to the research in progress; that is, they have formed only a part of a larger research programme. At the Institute the research is centred on developing a method which is applicable to all situations and not just to a few.

The solution which is proposed here is a two-stage process. Fig. 1 illustrates a flow diagram of the archaeological excavation process, with the two stages of the solution marked 1 and 2. Once the data have been excavated and recorded on context sheets or in notebooks they are then initially stored on to floppy disk in a flexible format and usually in separate datafiles. At this stage (1) the data are not sorted or analysed in any way, but provision has been made for the data to be extracted for analysis, as well as for correction and addition. The second stage involves designing a data-base schema, that is, a plan or key showing how the different data are to be organised in relation to one another. The data are then transferred from the floppy disk datafiles into the database structure.

COMPUTER RECORDING OF ARCHAEOLOGICAL EXCAVATIONS

The DBMS then allows data to be extracted for analysis, to aid the archaeologist in writing the excavation report. This process will now be described in more detail.

Data capture

The recording of data on to floppy disks using a microcomputer requires data-capture programs to be written. Until recently there were few suitable commercial programs for use by archaeologists, and even those now available often impose their own constraints, which make them unsuitable. This has meant that archaeological units and departments have developed their own data-capture software, but these are often designed around a particular recording system or the set of data to be stored. The data-capture program developed by the author has attempted to avoid putting too many constraints on how or what the archaeologist can record.

The first version of the data-capture program was designed by Dr Graham when the Institute first acquired a microcomputer in 1976. The aim was for microcomputers to be more efficient than on-site terminals which were expensive to run and slow to operate. The program was written in Z80 Assembly Language for a Zilog Software Development System, and one of the first users of the system was the Mucking Post-Excavation organisation through whom the author became involved in this work. They used it to store plan descriptions and context and identity data about flint and charcoal finds. Since then the author has taken over the development of the system and has partially redesigned the data-capture program as well as writing a retrieval program. This latter was also written in Z80 Assembly Language, but both programs have now been rewritten in Pascal which enable them to be used on machines other than Z80-based microcomputers.

The question sourcefile

The two programs as they are presently set up to store and retrieve data use what is called a question sourcefile. This is basically a list of statements, or rather data field descriptions which form the 'blueprint' of how the data are organised in the datafile. The data values stored for one cycle of a list of 'questions' is called a datafile record, and the datafile contains many of these. The position of a data field within the datafile record is determined by the 'question' with which it is associated, as well as by the order in which the 'questions' have been answered. In archaeological terms a data field could be represented by a single attribute of a feature or artefact, whereas the datafile record is all the data relating to a particular feature or artefact. However, exactly how the question sourcefile is used by the archaeologist entirely depends on the archaeologist: the sourcefile only provides the means to 'capture' the data.

It is intended that the question sourcefile can be written by anyone who has data to store on to computer, including those who have little prior experience of computing, except possibly for the knowledge necessary to use a text editor.

The general format of a data-field description or 'question' is:

.
xy: Data field name
?

The question sourcefile is made up of a list of such statements where the full-stops are the delimiters which separate one description from the descriptions preceding and succeeding it. Each data-field description is identified by a unique two character label, 'xy' where 'x' can be any number or letter except 'C', and 'y'. If the first character is 'C' the programs assume that until the next delimiter there is some free text. This text is not used by either of the programs but it is intended to be used for explanatory notes on the 'question' list, usually for anyone who is unfamiliar with the data that are stored. The value of 'xy' label is given by the user and this is primarily for use by the programs. Each field is also identified by a name which describes to the archaeologist what the data values really mean. The question mark represents the data-field type code letter which informs the programs what type of data they are to expect for that field. An example of the data field description is:

.
03: Northing Co-ordinate
N
.

Data types

There are five principal types of data recognised by the two programs. These are more to do with computer concepts of data than with archaeological concepts and hence make the system adaptable to different recording systems. They are:

- Numerical — Decimal or integer numbers.
- Keyword — Up to four characters.
- Word — Up to and over four characters, but not including the space character.
- Text — Up to 255 characters, including the space character.
- Alphanumeric — A combination of letters and numerals.

These are further sub-divided so that during data capture checks can be carried out by the program, to see that the correct data are being stored. Thus for numerical data there are three sub-types recognised by the following codes:

- N — Any decimal or integer number.
- R — Any decimal or integer number between specified limits.
- Q — A sequence of real or integer numbers, separated by commas or spaces, of up to 255 numerals and separators.

With numeric data various non-numerical symbols are also allowed, 'c' is used for circa or approximate, '+' and '-' are of course also allowed while the latter can also be used to represent a range, e.g. 43-50. Other symbols include ':' and '/' so that ratios can also be stored. If the value is unknown then the character 'X' can be inserted. With

COMPUTER RECORDING OF ARCHAEOLOGICAL EXCAVATIONS

the 'R' type data the lower and upper limits are stored in the question sourcefile in the form:

.
02: Northing co-ordinate
R (0,1000)
.

Here the data field must contain a number between 0 and 1,000 inclusive.

The keyword data, which has the code letter 'K', is designed to minimise the amount of storage space used for the recorded data, which is particularly useful if there is shortage of space, e.g. as on a 5¼ in floppy disk compared with 8 in disks, or if there are large quantities of data to be stored. The keyword list is stored in the question sourcefile, usually with some explanation, e.g.

.
15: Pottery type
K BKR — Beaker
 BKT — Bucket
 FLG — Flagon
.

Only the keyword is entered at the keyboard during the data-capture session and stored in the datafile but *only* if it is one of those specified in the list, otherwise an error is signalled and the 'question' repeated. Word data, type 'W', are similar to this but they allow the storage of longer descriptions, so that the above data field description could also be written as

.
15: Pottery type
W Beaker
 Bucket
 Flagon
.

With the 'W' type it is also possible to store double words by hyphenating them, e.g. Barrel-beaker.

Keywords are usually abbreviations or acronyms, and a particular keyword should not be repeated within a keyword list, as only the first occurrence of this will be recognised. However, it is possible to have the same combination of characters in different keyword 'question' lists, that is in two different data fields and possibly meaning something different, although this might lead to some confusion. The programs make no check on the lists of keywords in the question sourcefile to see if there is any repetition, and so it is advisable that a separate list be kept of all the keywords used to stop unnecessary repetition.

There is a special keyword type which has the code 'M' which indicates to the program that this is a multiple keyword question. During data capture the question is repeated until the keyword 'N' or the symbol '-' is entered indicating there are no more keywords to be entered. This is particularly useful for descriptions when more than one keyword may apply to the object or context in question, in different combinations. In the datafile the keywords are separated by a plus sign, indicating that they are really a single field. Associated with the 'no more' keyword value in the keyword list is a goto statement which informs the programs which is the next data field label. The equivalent multi-word data type is the 'X' data field type. An example of a multiple keyword data-field description is:

```
.
17:  Parts present
M    RIM — rim of pot
      BODY — body of pot
      NECK — neck of pot
      BASE — base of pot
      N — none or no more (goto 18)
.
```

Another special keyword data type is the program control data field, which has the type code 'Y'. This allows loops to be introduced into the data. The question must be answered by the characters 'y' or 'n' for yes or no, and there is a goto statement associated with the field description as follows:

```
.
30   Another sub-cycle
Y    (if yes goto 15
      if no goto 01)
.
```

In the above example, if 'y' is entered the program associates the next data field with field label 15, whereas if the answer is 'n' the programs will begin a new data record.

It is also possible to use goto statements with most of the other types of data, in particular with the word and keyword type, where different answers may lead to different sets of data fields. Using the pottery type data field description above, this can be modified to be

```
.
15:  Pottery type
K    BKR — beaker (goto 20)
      BKT — bucket (goto 25)
      FLG — flagon (goto 30)
.
```


COMPUTER RECORDING OF ARCHAEOLOGICAL EXCAVATIONS

Thus, if the data item stored is flagon (keyword FLG) the programs automatically assume that the next data item stored is associated with data field 30. This and data fields beyond will contain data which are relevant only to flagons.

Text data, type 'T', is used to store data about an artefact or feature which cannot be coded or reduced to a single description.

Alphanumeric data, type 'A', is designed to accommodate those recording systems which use both letters and numbers, e.g. AB56, which are often used by archaeologists. The range of possible values is stored in the question sourcefiles as follows:

```
.  
0.5: Area code  
A   (A, C; A, Z; 0, 9; 0, 9)  
.
```

This will accept alphanumeric data in the range AA00 to CZ99.

The question list structure

The first data-field description in the list is preceded by a period. Before this there can be any amount of text, but excluding the full-stop. This text information may record the title of the question list, who wrote it and when the last changes were made.

The first data-field description must always have the label '01' or '1', and only one of these two labels may be used in the list. The last data-field description is succeeded by its period, followed by a dollar sign on the next line. After this more text may be stored in the file, and may include full-stops. None of the text before or after the 'question' list is used by either of the programs, but it can provide useful notes for those who are unfamiliar with what data have been stored.

The first data field of the datafile record is the record identifier, and this is usually a number. This enables each record in the datafile to be referenced by a unique value. There is a special data type for this, the 'S' or serial number type, which may be decimal or integer, and during data-capture, is entered by the user when it is first encountered. When it is found again, the program automatically increments the number and stores it.

The general question sourcefile structure can be represented as follows:

Question Sourcefile Title

Name of author

Last update:

```
.  
01: Record serial number  
S  
.
```

```
02: Data field description two  
?
```

```

.
03: Data field description three
?
.
.
.
.
.
.
30: Another sub-cycle
Y   (if yes goto 15
    if no goto 01)
.
$
More text explaining the question list

```

The 'questions' should be organised so that data fields which occur only once in a record come first in the list, and these should be succeeded by those data fields which can be repeated. An example of this is provided in appendix 1, which lists a question sourcefile used by Mucking Post-Excavation to record data relating to tiles. It is not intended to describe the archaeological content of this in detail or comment on it in any way (see Catton *et al.*, 1982, for a discussion of computing at Mucking Post-Excavation), but it is necessary to give some explanation of the way the data are stored. Artefacts from Mucking Excavation were recorded using a two-axis co-ordinate system, while depth was measured in 3 in spits. The layer in which the find was made was also recorded. Further, each co-ordinate pair was assigned to a feature, which represents the general context of the area in which the finds were made. These data are stored first as they occur only once in the tile records. Notebooks were used to record details of the excavation, and if there is a reference to the relevant context this is also stored, and since there may be more than one reference this follows the context data. The actual attributes of the tile, which were examined during the post-excavation phase, succeed the context data in the datafile record, as more than one tile can be found in a context. It is advantageous if context data are stored on computer while excavation is in progress, so that after excavation the record only has to be expanded. This was not possible at Mucking as the excavation began in 1965, and ended just after microcomputers appeared, so all the data had to be entered during the post-excavation phase. More is said below of how the tile data are to be stored finally.

The data-capture program

Once the question sourcefile has been written it is possible to use the data-capture program to store the data on floppy disks. The program begins by asking the operator for the name of the question sourcefile to be used, and then of the datafile to be generated in which the data will be stored. A new datafile is created each time the program is used

as a safeguard against possible disk writing errors or power failures, when data may be lost or corrupted. The program then stores the name of the question sourcefile used to store the data in the datafile, so that the retrieval program knows which question sourcefile is the 'blueprint'.

The program loads the list of data-field descriptions into the memory and generates a control table, which contains the labels and the data types for each of the fields, as well as the position of the descriptions in the memory. This increases the speed with which the program can find the 'questions' and the keyword and word lists.

The program then asks for the date and the initials of the operators who are recording the data. These are also stored in the datafile in case any problem arises about the data which have been 'captured'. Once this information is stored the data-capture cycle begins.

The data-capture program uses the data-field names from the sourcefile as prompts, but the labels are only used within the program and they are never displayed. The answers received from the console are checked against the information that was stored in the sourcefile, and if any errors are found these are signalled on the console screen and the 'question' is asked again.

There are various control features to the program. These allow the operator to display information regarding a particular question, to move back to a previous data field or to display the data so far entered for the current cycle. The information displayed about a data field depends on its type code, for instance the keyword list is displayed for a data field which requires a keyword. Similarly if a range was specified the program will tell the operator what the limits are. It is also possible to cancel a complete cycle of data, if it is found that there are several errors in it.

At the end of each cycle of data capture the program stores that record on to the disk, and begins asking the questions again. To end a capture session, a dollar sign is entered, and this tells the program to close the datafile and return the operator to the computer's operating system.

The datafiles

The datafiles are generated only by the data-capture program. They can be described as variable record length, variable field length sequential files. Each data field is separated from the next by a space character, except for text and sequential number data, where they are separated from other fields by new lines. It is possible to introduce new lines into the datafile after other question types, to give it a 'structured' format. There is no padding of fields nor is there any use of the null field. Thus in the example above, of the different sets of questions for different pottery types, for a flagon, there are not 15 null fields stored before the data item associated with question 30. This is to minimise the amount of space used in storing the data. Appendix 2 illustrates three records of tile data, stored using the question sourcefile in appendix 1. This illustrates a structured format, where after the context data each tile found is stored on two lines.

The raw datafiles are not intended to be used directly by any programs other than the retrieval program, but it can be useful to have a visually 'structured' datafile for quick reference by those who know the actual sequence of questions. A well-considered use of keywords and words is also easier to use than a numbered code.

The data-retrieval program

The retrieval of the data is carried out using a single program which allows the data to be extracted or examined in several different ways. Before the program is used on a particular system it is 'patched' so that it can use the console and printer facilities of that system to their best advantage. The program is controlled from the console, but can also talk to several disk drives and a printer. To make the program 'user-friendly' the program asks the operator questions and displays 'menus' if there are several alternative answers.

The data are stored in a structure which is decided by the data values themselves, given the range of possible structures allowed by the question sourcefile. Thus, in order to find a particular item of data, the program is designed to read every item of data stored in the datafile, and those which are required are extracted and stored separately and they are then used by the program. It is also possible to convert the layout of the data and produce new datafiles with a specific format, which may be required by analysis programs. Thus, there are options to change the records in the original datafile into fixed field, fixed length records, or else to give a fixed number of fields, but with variable field length. A particular example is the conversion of datafiles into a format that can use the GOS indexing package and here the data fields are given a tag so that they can be identified by that program.

The data can be displayed as a table of raw data, or translated, so that the question statement from the sourcefile is displayed with its corresponding data item. With keywords it is possible to display the explanation rather than the actual keyword. Appendix 3 gives an example of this type of display for one of the tile records in appendix 2. The data can also be displayed in a 'raw' form, or else a particular record can be examined in isolation.

When generating a new datafile or table, it is possible to select only certain questions, by their labels. It is also possible to specify certain conditions that a record must fulfil if data are to be extracted from it. Thus with the example question sourcefile it is possible to extract all the co-ordinates for a particular type of tile, which can be used to generate a dot distribution using a graph plotter.

The program does not have any sorting or analysis routines of its own, although future versions of the program may do some simple tasks. However, it is possible to retrieve data in a form compatible with sorting packages such as Supersort (Micropro, 1981). Editing of the datafile to make corrections must be carried out by a text editor or word processor, although editing will also be added to future versions of this program.

Long-term storage of the data

The datafiles are designed to minimise the amount of room taken up on a disk, but they can also provide a quick reference for those who designed the sourcefile. Once the data are on disk then it becomes theoretically possible to transfer them on to any other computer systems which have other forms of data storage. The structure of the datafiles does not lend itself to easy access to a particular data item, while large data sets are often found stored on a number of floppy disks. Thus, they need to be transferred to a system which can reach all the datafiles simultaneously, and which can be used to organise the data into a form which will allow easy access. However, floppy disks can be used to provide a reserve copy of the data, should anything happen to the original set.

The research at the Institute is presently concerned with how to store the data, recorded by the data capture program using question sourcefiles, so that any datum can be found relatively easily and simply. Thus, the problem is not only to decide how the data are to be stored, but also how to alter the structure of the original datafiles into the new form, preferably with as little attention from the archaeological computer scientist as possible.

Database Management Systems

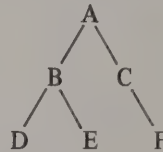
Database structures are one method of keeping data on computer-based media, but have until recently been confined to mainframes. A database in the strict sense can be defined as an integrated collection of data where the data are stored in such a way that natural relationships can be used to retrieve the data. Further, the data are stored in such a way that correction is easy, while several different uses can be fulfilled simultaneously. This is different from a file management system, in that the user is relieved of tasks involved in the actual physical storage and retrieval of the data. The database management system takes care of all that.

There are three principal types of database structure:

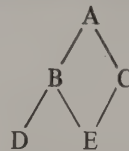
The relational database stores the data in tables which consists of a number of rows and columns. Each column is called a domain and each of the rows is called a tuple. In general if all the data for a particular set, e.g. the tile data, were arranged in a single table, there would be a lot of unnecessary repetition. With relational databases a process of normalisation is carried out which generates a set of smaller, simpler tables from one large table. This removes repeating groups of data and also makes updating the data and deleting incorrect data easier. This method has the advantage that retrieval of data can be carried out by mathematical techniques, including set theory.

A hierarchical database is where the data items are grouped into what are called record types. Each data item in a record type occurs only once relative to the others contained in that record. When these items are assigned values, the resultant group of data are called an occurrence of that record type. The different types of records are then organised into tree-like structures, where one record type can own many of another

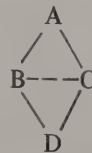
record type but an individual record occurrence can only be owned by one record. The set relationship is called a one to many or $1:n$ relationship. Individual data items have to be retrieved from records along specific pathways or branches. The whole design of the tree is called a schema. A hierarchical database structure can be represented by the following diagram, where A, B, C, D, E and F represent the record types:



A network database has the data organised in a similar way to the hierarchical database, but here it is possible for a record occurrence to be owned by more than one record type. CODASYL is a network database. An example of this structure is as follows



Another form of network database not only allows a record to be owned by more than one record type but also to own many occurrences of a record type. This is called a many to many of $n:m$ set relationships; a hierarchical database is in fact a special case of a network database. A network database which allows $n:m$ set relationships can be represented by the following diagram where A, B, C and D are the record types:



Although it would be possible to design a database management system exclusively for archaeological use, this is really unnecessary with the powerful and general commercial software which is now available. However, to make commercial database systems generally useful to archaeologists, it is necessary to develop programs which will enable them to be used without too much trouble. We are concentrating on writing such software for a commercial network type database system called MDBS version 1 (MDBS, 1980). The purpose is to tie this into the data capture system described above.

MDBS

This is a set of programs that allow a microcomputer to be used to generate a fully-fledged database management system. It is based on the CODASYL model of data

organisation (a brief description was given when describing a network database) but it is an extension of that because it allows many to many set relationships. It has a Data Description Language (DDL), which is used to describe the database schema, that is to specify what data items are found in a particular record, and to describe their set relationships. A Data Manipulation Language (DML) is also provided, and this can be used within programs written in computer languages such as BASIC or FORTRAN. At the Institute the DML is hosted in Pascal. It is also possible to obtain several support programs, which enable the user to recover a database should it become corrupted due to a hardware or software failure, or to restructure it without having to reload it from scratch. A query/report system can also be obtained, which allows access to the data stored in any database without using the DML. MDBS is a more complex package than other database management software, such as Dbase 2, and DataStar, where all the loading and manipulation is carried out by a single program. With MDBS you are given the 'bricks' to design and build a database and the programs to use it, whereas with Dbase 2 you are given a fully equipped 'building' which you then decide how to use.

Unlike some database software for microcomputers, some appreciation of database terminology is required to operate the package effectively. Literature is provided with MDBS which lets the user 'in gently' into database terminology, as this is not standardised. However, it is a very useful aid for actually learning about databases in general, as it makes it possible to design and develop a completely working database in a matter of hours, including time for possible alterations to the structure.

An example

Most of the data used to carry out this research comes from the Mucking excavation, Essex, which took place between 1965 and 1978. Although there are various problems associated with the excavation and the succeeding post-excavation, of how and when the data were 'computed', the use of the data capture system and the quantity of data available makes it suitable for developing the method.

Most of the data relating to tiles from the site have now been stored on floppy disk using the data-capture program, and this is sufficiently complex to illustrate the proposed development of a database.

Appendix 1 illustrates the tile question sourcefile, while appendix 4 shows how the questions can be organised into database records. Appendix 5 illustrates the tree-like nature of these data showing the type of set relationships which can be used. Appendix 6 gives the final DDL specification, were the data types are:

REAL — real numbers

INTE — integer numbers

CHAR — characters, followed by the number of characters to be stored

LOG — logical or boolean data (i.e. data items have the values true or false)

The sets are said to have an owner and a member record which can be stored in a particular order, e.g. FIFO indicates that the first datum stored will also be the first datum extracted; SORTED indicates that the values are stored in ascending order; System owned sets are the 'entrances' to the database structure. Any record type that is owned by the system can be accessed directly, otherwise it is necessary to follow a pathway through other sets.

This conversion was carried out manually, but it is intended that eventually the whole process will be carried out by one or more programs. The development of these programs can be divided into three stages.

The first stage is to develop a program that will take the DDL specification and the question sourcefile, and by matching the data items to the question labels will produce a table that can be used to transfer the data from the datafiles into the database. With regard to loading the data, there are two methods which can be used. The first is to store the data by taking each of the datafile records separately and loading them sequentially. Each of these are split into their constituent database records before the data are transferred into the database. This is probably the most convenient method from a time point of view, in that the program could be set up to store a complete datafile, and then be left to run, without it requiring any attention from the operator. The second method would be to extract data for a particular database record type from all the datafile records and store all the occurrences of that record type in the database. This would be repeated for all the record types, slowly working up the branches of the 'tree'. This would probably be more efficient in the amount of storage space used, and may possibly make retrieval faster, but the time taken to load the entire database would be greatly increased as several passes through the datafile would be required.

The second stage of development is to produce a program that will look at the question sourcefile and generate a DDL specification for a database schema. This would automatically generate the question label/data item matching table and would have to be interactive so that the archaeologist could modify the data structure produced, if this was necessary. The final stage of research is to expand this last program so that a group of sourcefiles can be combined into an integrated database structure. This would probably need a loading scheme similar to the second method described above.

Simultaneously with this development the principles of a generalised database retrieval program are becoming apparent. This would operate along the lines of a query/report system, but with certain additions in particular, it is intended to link the databases to a program which can control a digital plotter in order to produce histograms, pie charts and dot distributions of the stored data. The resultant set of programs would provide any archaeological organisation with a powerful tool for future archaeological research.

Conclusions

The above describes in outline the research program in progress, but it will not be possible to come to any major conclusions about its feasibility until the method has been

COMPUTER RECORDING OF ARCHAEOLOGICAL EXCAVATIONS

fully developed and tested. However, computer systems do allow for the storage of vast quantities of data in a form which enables the user relatively easy access and hence the potential for archaeology, a heavy producer of data, is great. Databases on mainframes are already in use, e.g. SARG in the United States and SATIN in France, but these tend to be on time-sharing systems and often require standard forms of notation and do not allow a great deal of flexibility. Although this latter point may be good for intersystem communication it is not necessarily a good thing for archaeology. However, what is certain is that computers, in particular microcomputers, will eventually become a commonplace item in archaeological research of all kinds. Further, the use of mini-computers solely for archaeological or museum work, as at the Museum of London, will make it easier for large quantities of data to be stored, and to be used. However, a detailed study of how the data are to be stored permanently, once they have been recorded on to floppy disk, has yet to be made. The above research is an attempt to solve the problems associated with long-term computer-based storage of archaeological data.

Acknowledgements

The author would like to thank Ruth Birss, Ian Graham and Professor Roy Hodson for reading previous drafts of this paper and suggesting improvements.

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Appendix 1 An example question sourcefile, for tile artefacts from the Mucking Excavation

Mucking, Essex, Question sourcefile for Tile
Sourcefile written by JPJCatton
Latest Update: 080781

C Questions recording context data

01: Record number

S

02: Northing co-ordinate (feet. inches)
R (-350, 2700)

03: Easting co-ordinate (feet. inches)
R (-200, 1300)

04: Level
R (0,99)

05: Layer description
T

06: Feature type

K	A	- Area	(goto 06)
	CL	- Clearing	(goto 06)
	D	- Ditch	(goto 07)
	B	- Barrow	(goto 08)
	CRE	- Cremation	(goto 08)
	GH	- Grubenhous	(goto 08)
	GR	- Grave	(goto 08)
	K	- Kiln	(goto 08)
	PG	- Pen gully	(goto 08)
	W	- Well	(goto 08)
	NAT	- Natural	(goto 09)
	PH	- Post hole	(goto 09)
	PIT	- Pit	(goto 09)

07: Ditch code

K	AEN	- A enclosure	(goto 09)
	BEN	- B enclosure	(goto 09)
	CEN	- C enclosure	(goto 09)
	RB1	- Romano-British Enc 1	(goto 09)
	RB2	- Romano-British Enc 2	(goto 09)
	N	- No code	

08: Feature number

N

C Questions recording reference data

09: Notebook number

N

10: Page number

N

11: Is there another reference
Y (if yes goto 09
if no goto 12)

C Questions recording attributes of tile finds

12: Tile number

N

13: Period of tile

K	MED	- Mediaeval	(goto 14)
	MOD	- Modern	(goto 32)
	RB	- Romano-British	(goto 15)
	SCR	- Scraps	(goto 31)
	UNC	- Uncertain	(goto 15)

14: Glaze

K	GLZ	- present	(goto 32)
	N	- not present	(goto 32)

15: Tile fabric

K	F1	- Compact fine brittle
	F2	- Sandy
	F3	- soft fine compact

16: Type of Tile

	TEG	- Tegula	(goto 17)
	IMB	- Imbrex	(goto 22)
	TUB	- Tubulus	(goto 22)
	BRK	- Brick	(goto 22)
	VOU	- Voussoir	(goto 22)
	UNC	- Uncertain	(goto 22)

17: Flange dimension

K	FLH	- height
	FLW	- Width
	N	- None or no more (goto 19)

18: Dimension (in mm)

N (goto 17)

19: Number of grooves along flange

K	GRV	- 1 groove
	GV2	- 2 grooves
	NGV	- no groove present
	UNK	- Unknown

COMPUTER RECORDING OF ARCHAEOLOGICAL EXCAVATIONS

Appendix 1 *continued*

20:	Type of flange cut out		26:	Number of teeth	
M	FLE — end		N		
	FEC — End + chamfer		.		
	SOC — Straight	(goto 21)	27:	Width of track (in mm)	
	LCO — Longitudinal	(goto 21)	N		
	LCC — LCO + chamfer	(goto 21)	.		
	N — None or no more	(goto 22)	28:	Pattern	
			K	STH — Straight	(goto 24)
				CUR — Curved	(goto 24)
21:	Cut out length (in mm ['+' if incomplete])			S/C — Straight and curved	(goto 24)
N	(goto 20)			MST — Multiple straight	(goto 24)
				UNK — Unknown	(goto 24)
22:	Corners		.		
K	COR — Corners	(goto 23)	29:	Dimensions of Tile	
	N — None	(goto 24)	K	L — Length	
				BDT — Breadth	
				T — Thickness	
23:	Numbers of corners			COM — Complete	(goto 29)
N				N — None or no more	(goto 31)
			.		
24:	Decoration/surface treatment/conditions		30:	Dimension (in mm)	
K	SIG — Signature		N	(goto 29)	
	DST — Distorted	(goto 24)	.		
	STD — Sooted	(goto 24)	31:	Total weight (in grammes)	
	COT — Cut outs		N		
	COB — Combing	(goto 26)	.		
	OIM — Organic impression		32:	Comment on tile	
	NAI — Nail hole		T		
	N — None or no more	(goto 29)	.		
			33:	Another tile in this context	
25:	Comment		Y	(if yes goto 12	
T	(goto 24)			if no goto 01)	
			.		
			\$		

Appendix 2 **Example of the data structure which results from using the question sourcefile in appendix 1**

```

231
932 711 1
N
PIT 244 166 N
1 RB F1 TEG FLH C30 FLW 30 N UNK FEC N N STD N N 50
FLANGE BROKEN OFF FROM MAIN BODY
Y
2 RB F1 IMB N N COM T 15 N 130
END PRESENT
Y
3 RB F2 TUB COR 1 COB 7 31 MST STD N COM T 15 N 150
N
Y
4 RB F2 TUB N COB 9 35 S/C DN N COM T 15 N 130
N
N
£
* 432
947 735 3
N
W 6 244 180 N
1 RB F1 UNC N N COM T 21 N 20
LARGE PEBBLE IN FABRIC
N
* £
653
945 738 4
N
W 6 244 183 N
1 RB F1 TEG FLH 43 FLW 27 N NGV LCC +5 N N N COM T 18 N 80
N
Y
2 RB F2 UNC N N COM T 15 N 10
N
N
£

```

The two stars indicate the datafile record used in appendix 3.

Appendix 3 **Example of the 'translated' form of data, using the retrieval program. See appendix 2 for the original data**

Record number: 432	Tile number: 1
Northing co-ordinate: 947	Period of tile: Romano—British
Easting co-ordinate: 735	Tile fabric: Compact fine brittle
Level: 3	Type of tile: Uncertain
Layer description:	Dimensions of tile: Complete
'N'	Dimensions of tile: Thickness
Feature type: Well	Dimensions (in mm): 21
Feature number: 6	Total weight (in grammes): 20
Notebook number: 244	Comment on title
Page number: 180	'LARGE PEBBLE IN FABRIC'
	End of record

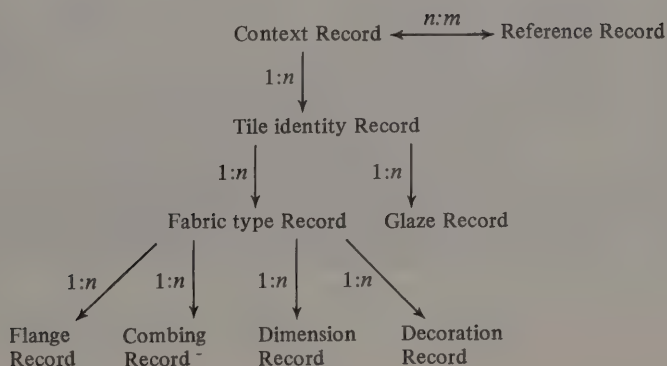
COMPUTER RECORDING OF ARCHAEOLOGICAL EXCAVATIONS

Appendix 4 Database records derived from the tile question sourcefile. The question labels are given in brackets

Context Record:		Tile type	(16)
Northing co-ordinate	(02)	Number of corners	(22)
Easting co-ordinate	(03)	Flange Record:	
Level	(04)	Height	(17)
Layer	(05)	Width	(18)
Feature type	(06+07 or 08)	Number of grooves	(19)
Reference Record:		Flange type	(20)
Notebook number	(09)	Cut out length	(21)
Page number	(10)	Combing Record:	
Tile identity Record:		Number of teeth	(26)
Tile number	(01+12)	Width of track	(27)
Tile period	(13)	Pattern	(28)
Weight	(31)	Dimension Record:	
Comment	(32)	Type of measure	(29)
Glaze Record:		Measurement	(30)
Present/absent	(14)	Decoration Record:	
Fabric Record:		Type of decoration	(24)
Fabric type	(15)	Comment	(25)

Appendix 5 Database structure for tile

The contents of each record are given in appendix 4.



Appendix 6 An MDBS DDL specification for the tile data

Record specifications

set relations

RECORD	CONTEXT		SET	CONTEXTS	AUTO 1:N
ITEM	NORTH	REAL		SORTED	
ITEM	EAST	REAL	OWNER	SYSTEM	
ITEM	LEVEL	INTE	MEMBER	CONTEXT	
ITEM	LAYER	CHAR 30	SET	REFS	AUTO 1:N
ITEM	FEATURE	CHAR 10		SORTED	
RECORD	REFRNC		OWNER	SYSTEM	
ITEM	NOTEBOOK	INTE	MEMBER	REFRNC	
ITEM	PAGE	INTE	SET	TILES	AUTO 1:N
				SORTED TILENO	
RECORD	TILEID		OWNER	CONTEXT	
ITEM	TILENO	REAL	MEMBER	TILEID	
ITEM	PERIOD	CHAR 3	SET	FABRICS	AUTO 1:N
ITEM	WEIGHT	INTE		FIFO	
ITEM	COMMENT	CHAR 30	OWNER	TILEID	
			MEMBER	FABRIC	
RECORD	FABRIC		SET	GLAZED	AUTO 1:N
ITEM	FTYPE	CHAR 3		IMMAT	
ITEM	TTYPE	CHAR 3	OWNER	TILEID	
ITEM	CORNERS	INTE	MEMBER	GLAZE	
RECORD	GLAZE		SET	FLANGES	AUTO 1:N
ITEM	PRESENT	LOG		FIFO	
RECORD	FLANGE		OWNER	FABRIC	
ITEM	HEIGHT	INTE	MEMBER	FLANGE	
ITEM	WIDTH	INTE	SET	COMBS	AUTO 1:N
ITEM	GROOVES	CHAR 3		FIFO	
ITEM	TYPE	CHAR 3	OWNER	FABRIC	
ITEM	COLENG	INTE	MEMBER	COMBING	
RECORD	COMBING		SET	DIMENSNS	AUTO 1:N
ITEM	TEETH	INTE		FIFO	
ITEM	WIDTH	INTE	OWNER	FABRIC	
ITEM	PATTERN	CHAR 3	MEMBER	DIMENSNS	
RECORD	DIMENSNS		SET	DECORS	AUTO 1:N
ITEM	DIMENSNS	CHAR 3		FIFO	
ITEM	MEASURE	INTE	OWNER	FABRIC	
			MEMBER	DECORATN	
RECORD	DECORATN		SET	CONREFS	MAN M:N
ITEM	TYPE	CHAR 3		SORTED	
ITEM	COMMENT	CHAR 30	OWNER	CONTEXT	
			MEMBER	REFRNC	

E>

Using Computers at Billingsgate

by KEVIN FLUDE

The Billingsgate excavation has been in progress for about nine months and was funded jointly by the Department of the Environment, the Corporation of London and the Manpower Services Commission. The site is of prime importance to the understanding of the history of the City as it is situated in one of the few areas where there is chance of finding evidence of the post-Roman and early/middle Saxon periods. The scale of the excavation is such that the amount of information recorded can justify the use of computers to help the recording and analysis of the data.

The Museum of London's Department of Urban Archaeology has recently purchased five microcomputers and a minicomputer system, initially to handle data from the Billingsgate Excavation. This work is being supervised by the author, with the help of Dr Ian Graham of the Institute of Archaeology.

The five microcomputers are Sig/net Z80 systems, built by a British company, Shelton Instruments. Four of the systems have twin 5¼ in floppy disk drive, either single or double sided, storing 170 kbytes of data per side. The fifth system has one double sided floppy disk, and a 5 Mbyte 5¼ in 'hard' disk. Two of the micros are used on the site for data entry; this includes the hard disk system, which has a graphics

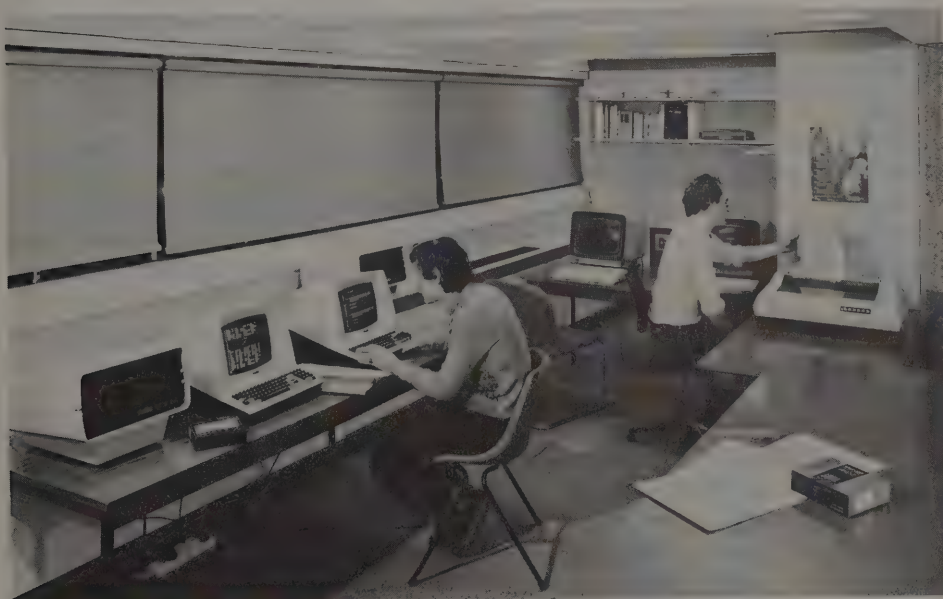


Plate I The Museum of London Computer Facility. On the left is Dick Malt using the Sig/net microcomputers. In the background is Kevin Flude using the PDP-11 minicomputer. (Courtesy of Museum of London.)



Plate II The Billingsgate Excavation. Remains of St Botolph's Church undergoing excavation can be seen in the top left-hand corner of the excavation. The site microcomputers are housed in the huts at the front. (Courtesy of Museum of London.)

terminal for the interactive display of context plans. Another two micros are in the Finds Department where, under the direction of Alan Vince, they are being used to transform the recording of finds. The last microcomputer is kept in the Museum's Computer Facility where, among other things, it is used to transfer data from floppy disks into the minicomputer system.

It is in this minicomputer that most of the data processing is carried out. The system consists of a DEC PDP 11/23 with 256 kbytes of memory and a 96 Mbyte disk. This configuration at present supports a maximum of four simultaneous users, running the Xenix operating system, a version of UNIX V7.

The team directly involved in computing consists basically of four people: Mike Lee and Vibhuti Patel funded as trainees by the Manpower Services Commission, and Alan Vince and Kevin Flude supported by various Department of the Environment and developer-funded projects. A number of others are involved, mostly in data collection and error correction. The computing work is based on the standard recording system used in the Department of Urban Archaeology, on work done by the author for a part-time research degree on excavation databases (supervised by Dr Graham), and on the archaeological experience of the computing team. Programming support is also provided by the Institute of Archaeology.

Data entry

Data entry on site is carried out using ADER, the package for Archaeological Data Entry and Retrieval developed by Jonathan Moffett and Ian Graham. The data are entered by the excavation team at Billingsgate as a normal part of their work. The philosophy behind this is that no distinction should be made between computer data entry and such standard techniques as plan drawing and context description, as they are equally important parts of the recording process. If using the computers was hived off to a separate sub-section then both recording, checking accuracy, and the awareness of the computer staff would suffer. Staff have taken well to the task after some initial resistance, and the standard of recording on site is certainly improving.

As far as possible the structure of the data entry programs has been made to mirror the tasks carried out by the staff at present. Thus finds information is recorded in a series of small data entry programs, as each task is completed. The excavation records (context sheets) are put in as one long program after each sheet has been filled in.

The data entry programs are controlled using a question sourcefile (QSf), as described above in the paper by Jonathan Moffett. QSfs are easy to write for simple data entry, but the simple control mechanism provided by the goto statement makes a large question sourcefile, such as that used for context data, rather convoluted.

The standard context recording QSf could be used for all types of context, but this would lead to over-complication and lack of clarity, thus special QSfs are in preparation for the recording of brick, masonry and timber contexts. However, we have yet to arrive at the proper balance between full and accurate recording and simplicity of use. As these types of context are rare on this site it is not efficient to adopt an over-complicated QSf for staff to learn. It has therefore proved sufficient to record these unusual contexts traditionally, and to computerise the results later, under the direction of an expert on masonry or timber features who can provide the correct nomenclature. Interpretation in the context record has deliberately been kept to a low level, and to those interpretations that can reasonably be made on site by the member of staff excavating a particular context. Additional, and often higher level, interpretations are added by the supervisor after consideration of all the evidence available about the context. For example, a cut might be described as a pit, but the further assignment of that pit as a well would only be made explicit at a later stage in the analysis. Of course, the excavator is perfectly free, and encouraged, to suggest this possibility in the interpretive comments section.

The programs for the computerisation of archaeological plans are still under development. However the graphics equipment will be used to locate newly excavated contexts in the stratigraphic sequence. The data held will be a digitised outline of the context and stratigraphic relationships of all context previously excavated. The digitisation will be done on site immediately after excavation of the context. The program will then work out the stratigraphic position of the context in relation to those previously excavated. The programs are written in Pascal and run on the microcomputer system.

At present we are in the process of computerising all finds records from past and

present sites, to enable work to proceed on finds analysis on all the data we have at present.

The raw data are collected on the Billingsgate site and input to the computer under several question source files:

Registered finds

Those finds that are recorded individually. Items recorded are: register number, context number, material of object, object name, period of object, storage information (location and required conditions), completeness, dimensions and comments.

Bulk finds

Those finds such as pottery and building material that are initially stored and recorded *en masse*. The information recorded is: context number, type of storage, number of boxes, find type, comments.

Findview

A program which records the joint decision of various experts on finds, conservation, etc., on registered finds which they view at a meeting held approximately monthly. The information recorded is: site code, register number, action to be taken, drawing/photo?, storage recommended, any change in initial identification, displayability of object.

If a junior member of staff has made a mistake in identification, or if the viewing reveals additional information, the program allows changes to be made to the object-name, the period, and the material given to the object. The displayability item is to record those objects the curators may be interested in for displaying in the Museum collections.

Spot dating

Finds specialists also view the bulk finds to provide a provisional date for site supervisors for contexts. For this we record: context no., type of finds, size of assemblage, earliest date, latest date, fabric form types and comments where known.

Computing in other departments

Question source files have been written for the environmental section, and some work has been done on recording photographic records. These and other projects will be continued after the bulk of the finds and excavation recording work has been finished. The computer is also beginning to be used to handle some aspects of administration, such as staff records. However, this has so far been relegated to second priority behind the academic objectives.

QSf or punctuated input?

Apart from the use of the ADER package we have had occasion to use other data entry methods. The most common of these is what I shall call 'punctuated' data entry, in which the fields of a data record are input in a fixed order, separated by a punctuation character, such as a semicolon; missing fields are shown by two adjacent semicolons, e.g.:

```
south;100;dark;grey;green;silty;clay
all;freq;;;flecks;charcoal
```

As can be seen from these examples, data entry is very compact, but not very easy to read and check. A comparison of this method with the ADER QSf method shows that the punctuated input method is quicker. However, in a large department where constant supervision of the data entry staff is impossible, the QSf method has the considerable advantage of guiding the data input and considerably reduces errors.

Data handling

The output of the ADER package is a series of CP/M files on floppy disks. These are transferred into the PDP 11 minicomputer using a communications part of the ADER package. This allows the transfer of a complete data file, or the selection of certain fields in each record, and uses the retrieval facilities of ADER.

File structure on the minicomputer system

Xenix uses a hierarchical directory structure to access data files. Members of staff or projects are given login names, and allocated file space appropriate to the work to be done. The very large capacity of our disk means that, at present, there is no shortage of file space. It is possible for group users to allow shared access to files. Thus members of the Department of Urban Archaeology have a directory of files called /usr/dua. The Billingsgate site has been given the login name of bill, and has all of its files held in /usr/dua/bill. Bill is a member of the dua group, and also has links to the finds and field section groups. Individual files can be made available to all users, or only to members of a particular group. It is also possible to protect files with pass-words, and to allow read-only access to prevent accidental corruption of important data.

Xenix is supplied with a large number of utility program packages, many of which are capable of handling data in the punctuated form described above. These packages are reasonably easy to use, and allow one the flexibility one could otherwise only get with much highly skilled programming effort, or by the use of a sophisticated database management system. In fact, the Xenix packages can be used together to form quite a powerful, do-it-yourself relational database system. However, unlike a full DBMS, the user-programs must know the names and physical locations of all the data files. To make use of the facilities of Xenix we made the decision to hold the data in the minicomputer in punctuated format. For simple QSfs it is an easy matter to transform the

data output by ADER into the required format. For large and complex QSfs it has proved necessary to write programs to interpret and transform the data.

The structure of directories under which the information is stored has been determined by the logical structure of the data, and the way in which it will be used, and to some extent by the peculiarities of the Xenix programs with which it will be analysed. Thus in the context file system we have separate directories for descriptions, administrative details, finds, and interpretation information. For the finds section there are directories for bulk finds, bricks, phasing, registered finds, and spot dating.

Splitting up the data like this provides benefits as each file is shorter, takes less time to process, and as only the information required for a particular purpose is present can make analysis easier. The advantages will be seen in the following section, where the Xenix packages are described. The disadvantages occur when it is necessary to look at the information in more than one directory, but the packages available with Xenix make this a small problem. We are developing new programs to remove even this difficulty.

Xenix systems programs

Xenix comes equipped with a large number of utility programs, and here only those which are most important to the day-to-day work will be described.

Stream editing

SED is a non-interactive editor, and allows one to prepare an editing script to alter data. We often collect data as keywords, usually a four-character code, consisting, if possible, of the first four letters of the full word. This reduces storage requirements and typing, but can be unintelligible for the uninitiated. SED allows easy and fast expansion of keywords, e.g. the SED command line:

```
s/;DEPO;/DEPOSIT;/p
```

will substitute ;DEPOSIT; for ;DEPO; throughout a file. The ;'s are necessary to prevent the characters DEPO in another word from being transformed into DEPOSIT, e.g. transforming the word DEPOT into DEPOSITT.

Sorting

Sorting is carried out on one line records, and the sort can be made on any or all of the fields in a record, e.g. the sort command

```
sort -nt, +0, +1 pots
```

will sort the file 'pots' on the first field (+0), and then records with equal values of the first field will be sorted on the second field (+1). The sort is numeric, and the fields are separated by commas (-nt).

Formating

A program called AWK (a pattern scanning and processing language) — named perhaps from the awkwardness of the manual — is a powerful program which can be used to reformat files. For example the AWK command line:

```
/;DEPO;/ (printf" DEPOSIT IS: %7s%7s%7s %8s\n", $3, $4, $5, $1)
```

will reformat and output all the lines in a file which contain the field ;DEPO;. Thus the input line:

```
123;DEPO;DARK;GREY;SILT;
```

would produce the output:

```
DEPOSIT IS : DARK GREY SILT 123
```

Briefly, the \$ stands for a field, the % says substitute the matching field here; and /n means insert end of line. Thus \$5 means field 5 which is substituted into the position where %7s is; 7s means provide a space for 7 columns for this character string.

This system makes data tabulation simple. The AWK program is also capable of simple arithmetic, e.g. the AWK line

```
tot = tot + $1
```

will keep a running total of the values in field 1.

Join

Join, of course, joins files, not end to end but line to line, and allows the user to specify which parts of each file are to be retained in the joined copy.

Thus an archaeologist requiring information on finds in particular soil types would ask for the soil description file to be joined to the finds file, perhaps requiring only the soil-type and finds-type from both files in his output file.

Searching

GREP (or Global Repeating Pattern scanning program) searches data files to find particular patterns of characters. The pattern can be defined in a number of subtle ways. In its most simple form it will find the occurrences of a word, e.g.

```
grep ';DEPO;' sitel
```

finds all the occurrences of the string ;DEPO; in the file sitel. The command line

```
grep '01:.*[Bb]uilding' inte
```

searches the file inte to find any lines that start with the characters 01: and contain the word Building or building. The .* in the command shows that any number of characters may lie between the 01: and the word building.

Combining commands and constructing program packages

These commands are separately powerful but when combined can form an extremely versatile system. Combination of commands is by two methods. The first is by means of 'pipes' which make the output from one program the input to another program. Thus:

```
grep 'PIT' data /sort -n +0 /awk -f awkprogram
```

This would find all lines containing the word PIT in the file 'data' and send only those lines to the sort program to be sorted on the first field numerically. This is then formatted according to the 'rules' specified in the awkprogram file, and printed out at the terminal. The ease with which these programs can be joined together using the pipe facility makes it possible simply to construct fairly sophisticated program packages. This enables one to handle 'one-off' non-standard applications quickly.

The second method of combining commands is to use the 'shell' or macro facility of Xenix. A shell file is a file containing commands. The commands can then be invoked simply by typing in the shell file name.

The shell file can also include instruction loops, parameter, substitution and other programming aids to make shell files extremely versatile and to allow interactive use.

The principal advantages of shell files are the reduction in typing time and typing mistakes and the ability to program sophisticated applications without the user having to know the details of the programs and files used.

Future uses of the computer system

It should be fairly clear that the system as outlined above is capable of using the data recorded in a number of ways and that given a request for specific type of data retrieval the computer staff can accommodate this without too many problems.

We are currently constructing the application programs for the data we are collecting. The major aims of the exercise are as follows: firstly to reduce the time taken to produce the excavation report, secondly to improve its quality, and thirdly to make the data collected available for other studies following the completion of the work.

The major contribution to speeding up the writing process comes from the fact that the data entry procedures present the supervisor with virtually complete cross-referenced and checked site records (Level 2 in Frere's nomenclature). Errors will have been mostly trapped by built-in program error checking and by the manual checking of computer printout. Secondly, we hope to use the computer to speed up the presentation of data for the supervisor. This will ease the quite substantial task of referring to the separate records in the numerous ring binder files in which site records are currently held. The graphics program will also have reduced the time taken to work out stratigraphic relationships.

The writing up process will proceed by extensive interactive use of the VDU, by consulting computer printout, and by using the graphics facility to produce interim

phase plans to test decisions as and when they are made. We also hope to use the computer to check the consistency of these decisions.

These uses will also improve the archive report and make it more simple to amend the report in light of new information. The archive report will be supported by fully cross-referenced catalogues, and will be produced using a word processing program. The quality of the report should be improved not only by the elimination of certain types of errors but also by the ability of the supervisor fully and simply to test the implications of alternative hypotheses. The graphics can be used to check the spatial implications of these hypotheses and can be used as an underlay for the speedy production of illustrations for the report.

After the completion of the Level 3 report we will be able to offer aspects of the database for use in further research. Information on sections of the data could be individually packaged for specialists. We will, for example, use the finds data for research on finds types throughout the city. Depending on finance we would like to use the database to investigate such topics as finds residuality, consistency of site records, comparison of finds assemblages from different types of deposit and many other studies. The investigation of residuality would involve a comparison of the pattern of finds found *in situ* with finds in residual contexts to investigate whether or not the residual finds conformed or not to the same pattern. Similarity of finds might suggest the residual finds are in original use local to their final resting place. A different pattern may suggest residual finds are 'imported' from outside.

The exciting aspect of this work is the ability to integrate all aspects of the archaeological work and to be able to ask fairly complicated questions and receive a speedy answer. We are thereby hoping to stimulate many fruitful lines of research.

Museum-based Archaeology — an Approach to Data Management

by BENJAMIN BOOTH

Introduction

(a) *The problem.* In common with many other museums the National Maritime Museum has realised the importance of properly documenting its collection, which numbers some two million items; this is being done through project PETREL (Roberts, 1980). The main thrust of this work is to document each object, so that its history, description and location are recorded. The Archaeological Research Centre is responsible for a number of such objects, and it additionally holds a greater quantity of excavation and research records. For these records to be useful they must be catalogued and indexed, and the system must be able to rapidly assimilate more data: a month's fieldwork might produce several thousand records, all of which must be made accessible. These records constitute a large archive, approximating (perhaps rather unfashionably) to Frere's Level III (Frere, 1975); whilst one might wish to debate whether this is a proper archive, and

whether such an entity should exist anyway, there is no doubt that this constitutes the bulk of our records, and that it is essential that it is properly organised.

(b) *The solution.* Having surveyed the scope of the information problem in the Archaeological Centre it was clear that the Museum's information retrieval system would be able to handle the cataloguing and indexing of these records; in using the GOS program package (MDA, 1980a) and MDA data standard (MDA, 1980b) it utilises the only serious tools available for this purpose. This paper does not describe the use of GOS in detail, as this is documented in numerous MDA publications, and its use in the museum is fairly standard (except that GOS has been implemented on a microcomputer). The MDA system provides the best facilities available, and has an increasingly wide community of users, so that a wide range of experience is available.

There are several adequate packages now available for data capture from excavations (Flude and Moffat, this volume; Jefferies, 1977) all of which have useful features. However, it was decided to use the program developed at Maxey by the author (Booth, Brough and Pryor, forthcoming), this option, being more familiar than the others. It was felt to be the best means of combining rapid data entry and communication with the GOS package.

It is possible that all of these tasks could have been performed by the GOS package, but the microcomputer version of GOS is still in its prototype form, and the microcomputer we have for fieldwork is not yet capable of running GOS; therefore the strategy adopted was to use these two well-tried systems and to develop communication between them.

2. Outline of practice

(a) *Data creation and data capture.* It is intended that data entering the system should do so in a form which will not require manual remodelling before it enters the powerful manipulative environment of the computer. It is possible to see how so-called improved data-processing could lead to extensive duplication and inefficiency; information about an object might be collected in a haphazard fashion by a curator, this would be passed to the Information curator, who would reformat these data according to the data standards, this reformatted data would then be passed to a data preparation typist, who would present this information to the computer in a form which can be understood. This same data would have been reproduced four times. Instead of such wasteful duplication of data it is intended that data should be reproduced the minimum of times before it reaches the computer. To achieve this each curator is made aware of the data formats — how the data should be arranged — and is given an appropriate medium for collecting this data; either a printed form or an interactive package for directly entering the data to the computer.

Records are presented to the information system by a person competent to understand the nature of the material being described; this might be an excavator describing

a stratigraphic unit, a curator describing a museum object, or a conservator describing the processes an object has been subjected to. Ideally this data would be entered directly at the computer terminal, but this is not always practical; hand-held computers are now available which would be suitable for field use, but they are prohibitively expensive; some particularly complex records (e.g. conservation) are best recorded on paper first — however, other relatively simple records, which are generated in the computer-friendly environment of the archaeological research centre may be entered directly.

(b) *Data capture — program package.* The package used is based on that developed by the author at Maxey but it has been modified to enable it to run in CBASIC under the CP/M operating system. At present the system is running on a CROMEMCO CS-1H. This microcomputer is a Z80 based machine, with a 5 Mbyte hard disk, and a 5 in floppy disk drive. It can easily be upgraded to the 68000 processor. The program package is capable of being run on any CP/M system with 64+RAM and two floppy disk drives. In essence it is a suite of programs which allow the user to define a file structure, to send data to this file, to edit and retrieve this data, and to pass the contents of the file, suitably tagged to another package (in this case GOS). In the present context the data capture package is not a stand-alone system which will perform all of the functions required of a museum/archaeological system, but it has been designed in a modular form, and simple indexing and statistics are easily added should these be required.

The package consists of two programs; SETUP defines the structure of the file to be used, and where applicable allows the creation of lists and permitted terms. Where consistency, and rapid coding and decoding of data are required a thesaurus file is created by SETUP. The MASTER program performs operations on the datafile which has been defined by SETUP. The files have fixed length records, enabling them to be accessed randomly by the operator. This has considerable advantages for the entry, editing and retrieval of records, but does not make particularly good use of disk space. However this package is primarily for the initial entry and editing of data, which will ultimately be passed to another package.

SETUP enables the operator to define the length of records, the number of fields per record, and the number of records per volume (files are subdivided into volumes, this being the amount of data conveniently stored on a floppy disk). For each field the operator is able to define its name, type (whether real, integer, string or coded text), the prompt which will occur for interactive input, and the caption which will precede the field when it is printed. Additionally, the operator is able to define what tag will precede the field if it is to be passed to another package. If the field is of the type coded text the operator defines which thesaurus table will be accessed by the field; and is also able to define and alter the list of terms in this thesaurus. The facility for coded text fields enables rapid data entry where there are only a few possible values for a field. A table of possible terms is displayed on the screen, each of which is numbered; the operator selects one by keying in the appropriate number. For editing and printing these are automatically decoded. This facility permits the input of data with few typing errors, terminology may be controlled, and disk storage is optimised. It has the disad-

vantage of limiting possible operator responses, and is therefore usually complemented with a free text note.

Once a file has been defined by SETUP the operator is able to interactively perform operations on this file by using the MASTER program. Single records or a range of records may be accessed. The program enables records to be written, edited, and printed out. Retrieval on all fields is possible. Records thus retrieved may be edited according to predefined instructions (for instance all Roman artefacts could be reassigned to the Later Iron Age), or they may be printed out.

Once the data have been entered and edited it can then be tagged appropriately, and passed to GOS; at present this involves taking the relevant floppy disk to the Cromemco system Z2D which is running GOS in the Information Retrieval Section of the Museum. With the planned upgrade to 68000 processor the GOS package will be able to run on the Cromemco CS-1H microcomputer in the Archaeological Research Centre.

(c) *Data manipulation – GOS.* The GOS program package has been designed specifically to process museum records, it is a powerful cataloguing and indexing tool, able to operate on a complex record structure, in which individual and group fields may be repeated as often as required. GOS is able to receive data in any consistent format, and using its editor may output data in suitable formats for other packages. Initially the implementation of GOS on the Cambridge University IBM 370 was used, but it has now been implemented on a Cromemco Z2D microcomputer, and it is intended that in the near future all the museum's computing will be done in-house. It has been written in BCPL, and compilers are now available for this language to be run on microcomputers.

3. Future directions and conclusion

With the planned upgrade to 68000 processor it will be possible for all operations to be performed on one machine. Such an economical and convenient system will doubtless lead to much more being done with the GOS program package than is done at present, in its role of cataloguing and indexing. In time GOS should become more user-friendly, and will be able to take on the data capture functions at present performed by the Maxey programs described above. It would be possible for all options to be performed interactively. The present cost of a hand-held microcomputer robust enough to be taken on fieldwork is about £1,000, although the Sinclair Spectrum, at £400 complete with disks might be used if it is provided with a suitable power supply. It is likely that the cost of such machines will decrease, whilst their power increases, so that data may be entered directly into the system in the field. Where something like statistics is required, which GOS is not at present able to do adequately, it is possible to pass data to other packages using the GOS editor – once initial cataloguing is complete these analyses will be useful.

The present strategy is geared towards the production of useful results, rather than ingenious innovation; wherever possible already tried procedures are being used and links

between these are developed where necessary. Already the effect has been to greatly improve the quality of data collected, and to increase the awareness of curators of the need for keeping proper and consistent records; in the long term all this data will be readily available to those who wish to use it; the problem will then be to effectively utilise this greatly increased volume of useable data.

Abstract

This group of four papers describes one aspect of the research into quantitative methods being carried out by staff and students of the Institute. After a short introduction by Dr Graham, three papers outline the differing approaches to excavation recording techniques being followed by research students.

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Experimental Studies in the Determination of Flaking Mode

by KATSUHIKO OHNUMA and CHRISTOPHER BERGMAN

Introduction

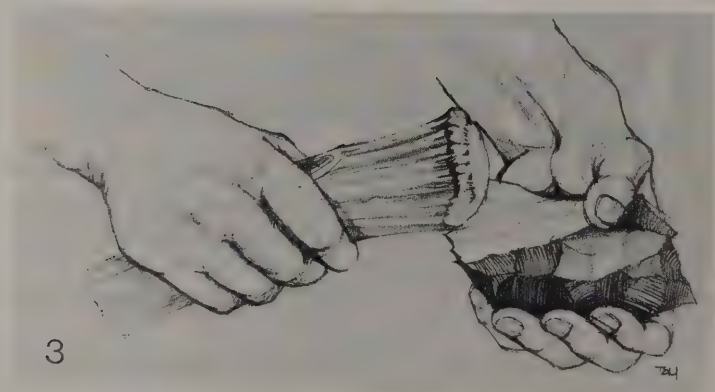
Many lithic technologists have recognised the importance of being able to identify the various kinds of tools used by Stone Age peoples in the manufacture of flaked stone artefacts (Bordes, 1948; Crabtree, 1967; Newcomer, 1975; Tixier *et al.*, 1980). These authors agree on the importance of the relationship between the tools used in manufacture and the technique employed in flaking stone; to produce certain types of artefacts appropriate flaking tools are needed.

Several authors have demonstrated that different stages in artefact manufacture sometimes require several different flaking tools and methods (Bordes and Crabtree, 1969; Crabtree, 1967; Inizan, 1980; Newcomer, 1971). With regard to biface manufacture Newcomer used comparisons of experimental and archaeological material to demonstrate that the thin bifaces of the Acheulean and the Mousterian of Acheulean Tradition were manufactured in several stages using different flaking tools and techniques. Percussors of quartzite and antler were used at the different stages of manufacture: roughing out, thinning/shaping and finishing. Inizan has pointed out the close relationship between the stages and flaking tools used in the Capsian blade(let) core reduction sequence which produces several classes of blanks.

These authors demonstrate that students of lithic technology cannot fully understand the flaking techniques used by Stone Age peoples without taking into account the various kinds of manufacturing tools used. It is important, therefore, to have a means for determining what kinds of tools were employed by prehistoric peoples in manufacturing flaked stone tools.

As early as the 1930s Barnes and Cheynier analysed the size of the butts and bulbs of percussion as well as various bulb types on Clactonian, Acheulean, Mousterian, Aurignacian and Solutrean flakes from France and England (Barnes and Cheynier, 1935). Barnes and Kidder studied the percentages of pronounced and diffused bulbs on plain and faceted butt flakes from the Mousterian, Perigordian and Aurignacian levels of La Ferrassie, France (Barnes and Kidder, 1936).

Bordes has discussed in detail the characteristic features of hard and soft hammer struck flakes (Bordes, 1947, 1948, 1961). The characteristic features recognised by



Bordes may be summarised as follows: flakes detached by hard hammers generally have large butts, clear points and clear cones of percussion, pronounced bulbs, clear conchoidal fracture marks and they sometimes have fissures which are more or less circular on the butt. Flakes detached by soft hammers have generally narrow butts which are often punctiform and/or lipped, no points or cones of percussion and diffuse bulbs. The fact that Bordes recognised large butts on hard hammer struck flakes and smaller, narrower butts on soft hammer struck flakes is probably due to the location of the percussion point and not to the material of the percussor.

In *Stone-Worker's Progress*, Knowles discussed the bulbar characteristics of flakes. He characterised flakes detached by hard hammers as having a salient bulb and a cone of percussion. Flakes detached by soft hammers have diffuse bulbs and no cones of percussion (Knowles, 1953). After analysing the relationships between the butt type, cone presence or absence and the bulb characteristics he was able to state that 260 out of a total of 282 Lower Paleolithic flakes from Biddenham Gravels in Bedfordshire, England, were most probably detached by a hard hammer.

It was Crabtree who emphasised the important role played by soft hammerstones among Stone Age people, stating that 'some hard hammerstones will become softened from repeated use until they have the same qualities as a soft hammerstone' (Crabtree, 1967). He also recognised that the 'diffusion of the bulb will depend, largely, on the amount of surface contacted by the hammerstone (Crabtree, 1967). Crabtree demonstrated that the 'edge-ground cobble technique', which uses a soft stone percussor, is effective for the detachment of blades with very small butts (Crabtree and Swanson, 1968).

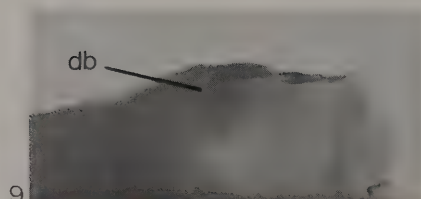
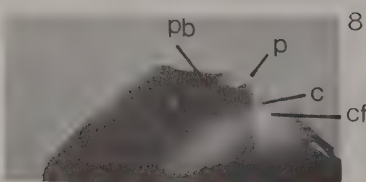
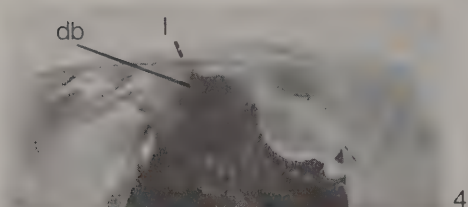
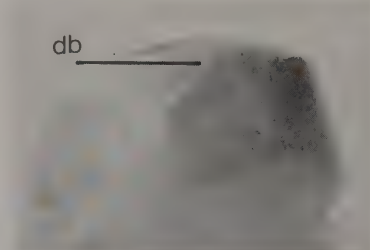
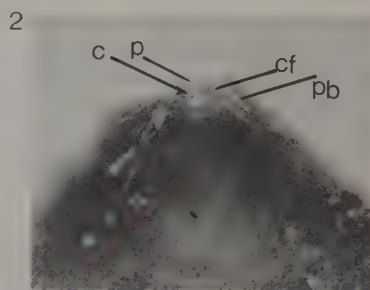
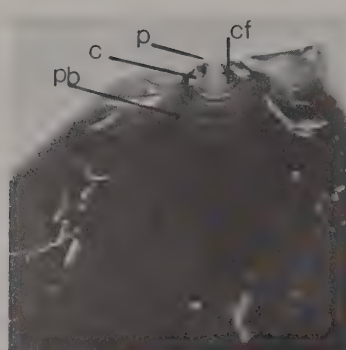
This concludes the introductory summary of the literature concerned with flaking mode. Beyond these general guidelines no one has provided a foolproof method of linking hammer hardness to flake features. Some authors have cast doubts on how accurate one can be in determining flaking mode. Through the experimental studies and examinations, described below, we were able to achieve a great deal of accuracy in determining flaking mode as well as understanding the effects of hammer hardness on the ventral surfaces of flakes.

Experimental studies and examinations

From May to November 1980 several experimental studies by Ohnuma and examinations by Bergman, Newcomer and Ohnuma were undertaken in order to investigate how precise one could be in determining flaking mode. The term mode is used here to mean the kind of flaking tools used within the stage by stage framework implied by

Plate I

1. Percussors used in the experimental studies and examinations: top, antler hammer; bottom (left to right), two sandstone hammerstones and a quartzite hammerstone. 2. Technique of flaking using a quartzite hammerstone; the point of percussion is well on to the striking platform. 3. Technique of flaking using an antler hammer; again the point of percussion is well on to the striking platform.



method (Newcomer, 1975). The following is a description of the experimental studies and examinations. In each of the tests performed the examinee(s) was not informed of how many flakes were produced by each mode, hard or soft.

Experimental study 1

Raw material: a single block of Brandon (Suffolk) flint. Percussors used: a quartzite hammerstone weighing 130 g and a Pere David's deer antler hammer weighing 210 g. Flaking technique: hand-held direct percussion; the point of percussion being well on to the striking platform.

Flakes chosen for analysis: 100 flakes detached by the quartzite hammerstone and 100 flakes detached by the antler hammer. A few flakes had cortex butts, but none were faceted. Only flakes with the same general shapes and dimensions were chosen, for the aim of this study was to investigate the characteristics of the ventral surface of flakes. Each flake was wiped with a damp cloth so that the features could be seen more clearly.

Each of the 200 flakes was checked for the following features:

1. Butt: lipped or unlipped
2. Point and cone of percussion: clear or vague
3. Bulb: pronounced or diffuse
4. Conchoidal fracture marks on the bulb: pronounced or indistinct

Results:

1. Butt: 54 out of 55 (98%) of the flakes with lipped butts were detached by the antler hammer; 76 out of 91 (84%) of the flakes without lipped butts were detached by the quartzite hammerstone.
2. Point and cone of percussion: 70 out of 70 (100%) of the flakes with clear points and cones of percussion were detached by the quartzite hammerstone; 87 out of 102 (85%) of the flakes with vague or no points or cones of percussion were detached by the antler hammer.
3. Bulb: 71 out of 72 (99%) of the flakes with pronounced bulbs were detached by the quartzite hammerstone; 93 out of 109 flakes (85%) of the flakes with diffuse bulbs were detached by the antler hammer.

Plate II

Scale: 3/2. 1. Experimental flake detached with quartzite hammerstone. 2. Levallois flake from Ksar Akil, Lebanon, 1947, level XXX (Middle Palaeolithic): hard hammer mode. 3. Experimental flake detached with antler hammer. 4. Experimental flake detached with soft stone hammer. 5. Blade from Ksar Akil, Lebanon, 1947, level XXX (Middle Palaeolithic): soft hammer mode. 6. Experimental blade detached with soft stone hammer. 7. Blade from Ksar Akil, Lebanon, 1937-8, level XXII (Upper Palaeolithic): soft hammer mode. 8. Experimental flake with faceted butt detached with quartzite hammerstone. 9. Experimental flake with faceted butt detached with antler hammer. Abbreviations used in Plate II: p, point of percussion; c, cone of percussion; pb, pronounced bulb; cf, conchoidal fracture marks; db, diffuse bulb; l, lipped butt.

4. Conchoidal fracture marks on the bulb: 42 out of 42 (100%) of the flakes with pronounced conchoidal fracture marks on the bulb were detached by the quartzite hammerstone; 100 out of 156 (64%) of the flakes with indistinct conchoidal fracture marks on the bulb were detached by the antler hammer.

After the above results were obtained, it was felt that two features — clear point/cone of percussion and pronounced conchoidal fracture marks on the bulb were good indicators of flaking with a quartzite hammerstone. By combining the other criteria their diagnostic value was increased. Analysis of the same flakes by using the above two criteria and combinations of the others was performed with the following results.

1. Lipped butt and diffuse bulb: 54 out of 54 (100%) of the flakes with these features were detached by the antler hammer.
2. Unlipped butt and pronounced bulb: 64 out of 64 (100%) of the flakes with these features were detached by the quartzite hammerstone.
3. Clear point/cone of percussion: 70 out of 70 (100%) of the flakes with these features were detached by the quartzite hammerstone.
4. Pronounced conchoidal fracture marks on the bulb: 42 out of 42 (100%) of the flakes with this feature were detached by the quartzite hammerstone.
5. Vague or no point/cone of percussion and diffuse bulb: 83 out of 95 (87%) of the flakes with these features were detached by the antler hammer.

It was observed that a cortex striking platform/butt altered the characteristics of the ventral surface of quartzite hammerstone detached flakes; the cortex absorbs the impact resulting in features closer to those of flakes detached by an antler hammer.

Examination 1

Ohnuma detached 200 flakes from a single block of Brandon flint. The flaking tools and technique were the same as those used in experimental study 1. One hundred flakes were made with each of the hammers.

1A

Examinee: Ohnuma using the method of combining criteria described above.

Result: correct (168); wrong (8); undecided (24): $168/176 = 0.954$ or 95% correct.

1B

Examinee: Bergman using the same method.

Result: correct (171); wrong (17); undecided (12): $171/188 = 0.909$ or 91% correct.

Comment: Ohnuma's greater accuracy is certainly due to the fact that he examined flakes that he had made a week before and as such the knowledge of the numbers of the flakes produced by each mode affected his result. Bergman on the other hand was not accustomed to using this method which may account

EXPERIMENTAL STUDIES IN THE DETERMINATION OF FLAKING MODE

for his slightly lower percentage of correct determinations. The results obtained by both examinees, however, convinced them of the potential reliability of this method.

Examination 2

Newcomer gave Ohnuma and Bergman 50 flakes which he had detached by direct percussion with a quartzite hammerstone and a red deer antler from a single block of Brandon flint. He made 25 flakes with each of the hammers; the examinees were not informed of this before analysing them. These flakes were examined with the method described above.

2A

Examinee: Ohnuma.

Result: correct (46); wrong (4); 92% correct.

2B

Examinee: Bergman.

Result: correct (46); wrong (4); 92% correct.

Comment: It was quite interesting that three out of the four flakes that could not be correctly identified by the two examinees were the same.

Examination 3

Bergman supplied Ohnuma with 100 flakes which he had detached from two nodules of Grimes Graves flint with sandstone hammer and red deer antler. The technique of flaking employed was hand-held direct percussion and the point of percussion was well on to the striking platform resulting in flakes with large butts.

3A

Examination of 50 flakes of coarser material.

Result: correct (29); wrong (18); undecided (3); 62% correct.

3B

Examination of 50 flakes of finer material.

Result: correct (28); wrong (20); undecided (2); 59% correct.

Comment: It is clear from the results that a soft stone hammer produces very similar ventral features to an antler hammer. It is very difficult to separate flakes detached by these two types of soft hammers with any degree of certainty.

Experimental study 2

Ohnuma detached flakes with faceted butts from large flakes of Brandon flint. The flaking tools and technique employed were the same as those in the previous experiment

by Ohnuma. The type of faceted butt was convex composed of multiple facets. One hundred flakes were detached with the quartzite hammerstone and 100 with antler hammer. These were examined using the same criteria as experimental study 1. Below are the characteristics of the ventral surface of both the quartzite hammerstone and antler hammer detached flakes with faceted butts.

1. Quartzite hammerstone detached flakes with faceted butts (100):
 1. Butt: lipped (1); unlipped (92); unclear (7).
 2. Point of percussion: clear (70); rather clear (1); vague (9); unclear (20).
 3. Cone of percussion: clear (8); rather clear (41); non-existent (35); unclear (16).
 4. Bulb: pronounced (10); slightly pronounced (44); diffuse (32); unclear (14).
 5. Conchoidal fracture marks on the bulb: pronounced (27); slightly pronounced (34); indistinct (29); unclear (10).
2. Antler hammer detached flakes with faceted butts (100):
 1. Butt: lipped (38); unlipped (40); unclear (22).
 2. Point of percussion: slightly clear (4), slightly clear due to the intersection of two facets (50); vague (42); unclear (4).
 3. Cone of percussion: non-existent (100).
 4. Bulb: diffuse (98); unclear (2).
 5. Conchoidal fracture marks on the bulb: non-existent (76); indistinct (24).

It was observed that each of the five ventral surface features of these 200 flakes were essentially the same as that of the previously examined flakes without faceted butts. The quartzite hammerstone detached flakes, however, generally had ventral features which were less prominent than those flakes with plain butts. Some of the quartzite hammerstone detached flakes with faceted butts could not be distinguished from those detached by the antler hammer. It was also observed that about half (56%) of the antler hammer detached flakes with faceted butts had slightly clear points of percussion most probably due to the intersection of two facets just at the point of percussion. Therefore it was felt, with regard to these flakes, one should consider also the presence or absence of the cone of percussion.

Examination 4

Fifty flakes from among those produced for experimental study 2 were examined by Bergman using the combined criteria described above. His results were very close to those of the previous examinations: correct (46); wrong (3); undecided (1); 94% correct.

Conclusions

The following conclusions have been reached as a result of the experimental studies and examinations.

In determining the kind of flaking mode (hard or soft) used by Stone Age people each of the following criteria proved valuable: (1) clear point and cone of percussion for the hard hammer mode; (2) pronounced conchoidal fracture marks on the bulb for the hard hammer mode; (3) unlipped butt and pronounced bulb for the hard hammer mode; (4) lipped butt and a diffuse bulb for the soft hammer mode; and (5) vague point/cone of percussion and a diffuse bulb for the soft hammer mode.

These criteria were established using extremely fine-grained flint from Brandon and Grimes Graves, Suffolk, England. It is recommended, in the application of this method to archaeological lithic assemblages, to use only the pieces made of fine-grained flint.

As was made clear by examination 3 the ventral characteristics of flakes detached with a soft stone hammer are difficult to distinguish, using the method described in this paper, from those detached by an antler hammer. It is probable that archaeological flakes identified as having been detached by a soft hammer include flakes detached by a soft stone hammer.

Due to the results of these studies we feel that percussors should be divided into two groups: those which are as hard as the material being flaked and those which are softer. In the case of fine-grained flint, these two groups can be easily separated on the basis of the ventral features of detached flakes. A more difficult task would be to recognise different types of percussors, such as soft stone or antler, belonging to the same group.

Acknowledgements

Thanks are due to M. Newcomer for his valuable advice and encouragement; to G. de G. Sieveking for supplying the flint from Grimes Graves; to T. Imai for his drawings.

Abstract

The paper describes a series of experimental studies and examinations made to determine how precisely the kind of flaking tools used in flint working can be identified from the product. As a result it proved possible to offer a series of criteria which should be useful for the determination of the flaking mode. The products of percussors which were harder than the material being flaked are easy to distinguish from those which are softer than the material, but it is more difficult to recognise percussors of different materials — e.g. soft stone and antler — belonging to the same group.

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Whether Pits be Vats: Some Further Interpretations of Subterranean Features

by CHRISTOPHER EVANS

Since Bersu dismissed the concept of pits as Iron Age dwellings and instead postulated their use as a storage facility (Bersu, 1940), there has been a profusion of storage-pits in archaeological literature. Alternative explanations derived from ethnographic sources concerning the function of pits have been explored elsewhere and these need not be repeated here (Binford, 1967; Ellison and Drewitt, 1971). Rather, I intend to consider the possibility of pits functioning as vats or subterranean receptacles, and to describe an example of their employment as such which was recently recorded during the course of fieldwork in Iran.

Though the ethnographic examples used in this brief paper will be drawn from Western Asia, their implications for the archaeological interpretation of subterranean features need not exclusively pertain to that specific region. Indeed, Bersu suggested that the shallower pits of his group A found at Little Woodbury, may have held sunken receptacles such as casks, baskets and tubs (Bersu, 1940: 43), and similarly, on a number of British Iron Age sites some smaller, and often clay-lined pits have been interpreted as water storage tanks (Ellison and Drewitt, 1971: 184). While environmental and geological factors will obviously influence the functional possibilities of these features, they are not necessarily absolute constraints as we are concerned with specialised activities which usually are of a limited duration. What should rather be the determining criteria for the archaeological interpretation of subterranean features is the regularity of shape and depth of the feature, the depth of ground water present during the life of the site, and the economic activities associated with the cultural context of the features in question.

In conjunction with the Royal Ontario Museum's 1978 excavations at Qal'Eh-i Yazdigird in the Kermanshah province of Western Iran (Keall, 1976, 1977, 1979), a programme of ethnographic research was conducted which included a study of the recently abandoned, black tent campsites of the Zardeh basin's transhumant Kurdish population (Evans, 1980, 83). This survey was undertaken when the transhumant sector of the Basin's communities had returned to their 'home' valley from the Dalahu uplands in the latter half of September. At this time the tents of the transhumant pastoralists were pitched in the harvested and fallow fields in the south-east quarter of the valley and lay

at a distance of some 500–1,200 m from the main village in the valley, Zardeh. In total, the remains of 30 former tent sites, which had been organised in six encampments ranging in size from two to eight tents, were recorded and surveyed according to what was essentially an archaeological perspective.

Associated with one of the smaller campsites were a number of depressions which suggested subsidence within back-filled pits and these features were excavated by the author. One of these subsidences proved to be a depression created by the removal of a large rock which apparently had been used in the construction of an adjacent terrace wall, and another was a shallow pit which contained hearth dumpings. However, two further pits were excavated and these proved to have been carefully dug and relatively straight-sided features which had been cut through the limestone bedrock to depths of 70 cm (feature 1) and 65 cm (feature 2) and which had diameters of 110 and 65 cm respectively. The functional volume of these pits is calculated as being approximately 0.372 m³ (f.1) and 0.239 m³ (f.2) and this would represent a maximum holding capacity of 372 and 239 litres accordingly. By their general size and shapes these two features are comparable with Bersu's group A pits (Bersu, 1940: 50).

The fill of the two deeper pits was composed of loose soil, large stones, pieces of plastic, chaff, dung, ash and charcoal. In one pit lenses of roots and small twigs were both found in the fill and in a layer of compact reddish clay which lined the bottom of the pit. From the obvious attention which had been given to the original excavation of these two features, and from the twig-work lining found at the bottom of one of them, it seemed logical to interpret these features as being some form of storage-pit. This interpretation was also influenced by the fact that grain storage-pits are a recognised

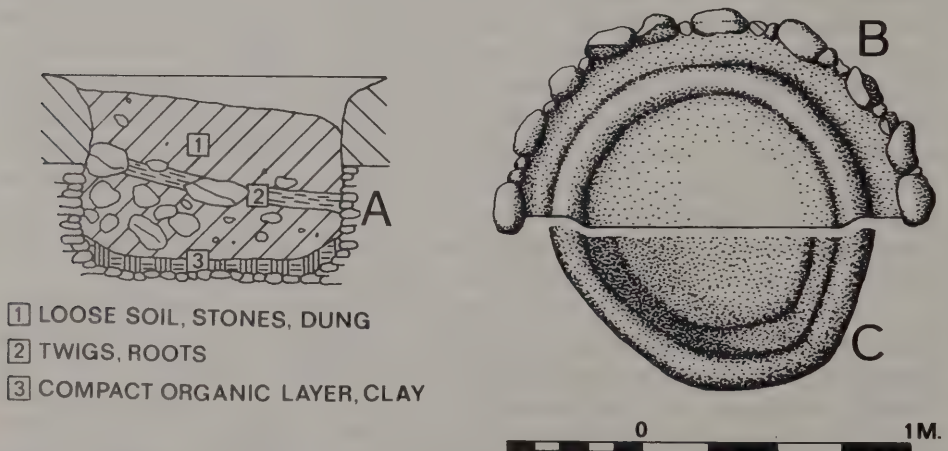


Fig. 1 Biscuit-making vat/feature 1 from Qal'Eh-i Yazdigird, Iran. A. Section through back-filled vat. B. Reconstruction of the vat sealed with a plastic sheet. C. Reconstruction of the clay-lining of the vat.

feature of present-day villages in the Zagros region (Watson, 1979: 125, fig. 5.4). It was therefore surprising to learn from the site's recent occupants that these two features had, in fact, functioned as vats. We were informed that these features had been lined with clay and after this had dried cracked wheat and buttermilk (*dugh*) was poured in. This mixture was then sealed with a plastic sheet which had been pinned down around the edges of the vat by a ring of large stones. Formerly, a goat-skin would have been used to seal the vat. The buttermilk and wheat mixture is left in the ground for three days after which time it is removed and shaped into biscuits. These sour-wheat biscuits are then left to harden in the sun while placed on a bed of uprooted bushes (*torshoja*) and are afterwards gathered into sacks in which they are stored for winter consumption.

At the completion of this biscuit-making process the surface of bushes, which had been used for drying, was thrown into the bottom of the pit. It was this site clearance activity which produced the twig-work lining that we recognised during our excavation of the features. The greater part of these features had been refilled with soil and a matrix of campsite debris (ash, charcoal, dung, chaff, feathers and plastic, etc.). The large stones which had been used to anchor the vat's seal had also been deposited within them; 18 and 15 such stones were found in features 1 and 2 respectively. It is important to note that only in the bottom of feature 1 was the clay-lining of the vat preserved and this presumably was due to the organic material which sealed and eventually mixed with the clay. The erosion of the clay-lining can be accounted for by the seepage of ground water during winter rains and by the fact that it had not been fire-hardened. This rapid erosion after only a period of one year (see below) suggests that the recovery and recognition of all non-fire-hardened clay-linings in archaeological contexts is doubtful.

The attention given to the back-filling of these vats reflects the symbiotic relationship which exists between a field's owner and its temporary transhumant occupants. While there is no direct payment made for the use of the field as a campsite, nevertheless, its owner is rewarded by the clearance of field-stones from his land and by the spread of manure which accumulates from animals kept on the campsite. In fact, the back-filling of these features by campsite members had been so thoroughly done that we were only able to recognise last year's vats due to their subsidence. It was only from our informant that we eventually learned that this season's vats had passed unnoticed by us and lay beneath an even spread of dung on the opposite side of their tents. When we inquired why new vats had been dug instead of re-using last year's, our informant replied, 'New is better'.

It is relevant to note that the use of vats and the process of sour-wheat biscuit-making is not exclusively confined to the transhumant population in the valley. The same process of biscuit production is also undertaken in fields which lie adjacent to permanent homes in the valley. This procedure should be considered in the context of post-harvest production, for it requires both the grain from the valley's fields and substantial quantities of buttermilk. That this buttermilk is only widely available upon the return of the valley's transhumant flocks in September, indicates that this procedure of biscuit preparation is only feasible when the arable and pastoral sectors of the Zardeh com-

munities are reunited. In other words, it is both a post-harvest and post-migration production. This is not to imply, however, that the possible use of subterranean vats for food preparation is necessarily restricted to agricultural and/or pastoral economies. Certainly, it is conceivable that uncultivated food resources, including wild cereals, could have been processed for indirect consumption and storage as beverages, gruels, cakes or biscuits, involving the use of a similar subterranean facility.

Thus far, I have been unable to find any direct parallel to this process of vat-made biscuits in ethnographic records. Traditionally in West Asia the domestic processing of milk by-products requires no more than skin bags for churning and storage, cooking pots, and cloth used for straining, though clay and wooden churns are used in settled communities (Ferdinand, 1969: 150–7; Watson, 1979: 110–11). However, among those pastoral nomads of Eastern Afghanistan who own large flocks, the production of buttermilk cheese (*patei krut*) also entails the construction of a temporary vat (Ferdinand, 1969: 151, fig. 6). In this procedure a standing square or an embankment of loose field-stones is built. A cloth is then spread over this stone frame which enables it to function as a shallow vat or settling tray. Boiled and thickened buttermilk is poured into this cloth-lined vat and there it is left to solidify while the whey is able to seep through the cloth and into the ground. When the *krut* cheese is half dry it is cut into small squares and these are later stored for winter consumption, as fresh buttermilk is not available during that season. Archaeologically, the physical remains of this procedure would consist only of the stone alignment of its frame.

In Persia vats have traditionally been used for a number of domestic and craft purposes. Large earthenware glazed vats which are sunk into the ground function as lime and curing pits during the preparation of hide, and the actual tanning of hides is done in deep, wood-lined, brick-built pits. Similarly, bones which are to be used for inlay decoration are first prepared in standing earthenware vats where they are left to bleach for some three months in a solution of quicklime. Vats are also recorded as being used in the dyeing and preparation of textile fibres, in grape-mashing, and in the washing and filtering of minerals (Wulff, 1966: 93, 153, 193, 231, 300). One can expect to find all of the above mentioned craft activities in various archaeological contexts. The vats which are used in these production processes appear to have a standard height or depth that is generally in the range of 1 m. This size seems to be determined by practical considerations, such as the ability to reach to the bottom of a vat so that it can be emptied, and in the case of mass-produced earthenware vats (Wulff, 1966: 153), by its bulk and weight so that they may be transported without undue effort. When we are considering excavated pits as being vats, then depth is not only determined by access or feasibility of reach, but also by the effort required to dig a pit much deeper than 1 m when it would be more practical to dig a second feature. The advantage of using a lined pit as a receptacle rather than a clay vessel or a skin bag, is that it provides a large, easily constructed container which does not take-up living space and nor does it require storage when it is not in use. These factors are, of course, very important in relation to specialised seasonal activities, for a specific container may be only required for a few days of any year.

In this context it is also relevant to question whether all archaeological subterranean features were intended for only a single purpose or function. A feature that may have been a storage facility throughout the greater part of a year could conceivably have had secondary functions during specific phases of the food gathering, agricultural or pastoral calendar. Another factor in favour of subterranean vats is that they do provide relatively insulated storage, and clearly this factor is to be considered in the processing of milk products. Therefore, there are definite functional advantages in the use of subterranean containers and this is especially true for both densely populated communities, where living and storage space is restricted, and for migratory societies for whom the transportation of large rigid vessels is impossible. In a prehistoric context, the greatest potential for the possible use of subterranean features would be among those cultures who, though practising advanced food processing and/or craft production, have not developed a sophisticated pottery technology. This criteria could then include advanced hunting and gathering societies, pastoralists, and also Pre-Pottery and early Neolithic cultures.

These ethnographic examples of alternative explanations for subterranean features are not intended as a 'cautionary tale' for archaeologists. Although our apparent storage-pits did prove to be biscuit-making vats, they are none the less a facility or stage in a process of indirect grain storage. I believe rather, that the purpose of such examples is to suggest the potential for archaeological interpretation which is to be found in ethnographic research and records. As a resource, ethnography offers to the archaeologist a basis of cultural data which can stimulate alternative explanations for archaeological phenomenon, and against which we can question and re-examine our established interpretations of features and material distributions. It is not my intention to suggest that pits are necessarily vats, but rather to ask whether some pits may have been vats.

Abstract

In the course of recording a campsite of the transhumant Kurdish population of the Zardeh basin in Western Iran several pits were recognised and excavated. Two carefully dug examples about 70 cm deep appeared to the excavator to be grain storage pits, which are used in present-day Zapros villages. According to the late occupants of the site, however, they were used as vats to hold a mixture intended for the making of biscuits. Vats of various types are used in Iran for a number of different purposes. This is clearly one function which should be kept in mind when considering the possible use made of pits found on any archaeological site.

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CHRISTOPHER EVANS

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Book Reviews

ROYAL COMMISSION OF HISTORICAL MONUMENTS, ENGLAND. *Long Barrows of Hampshire and the Isle of Wight*. London, HMSO, 1979. xxxvi + 77 pp., 62 figs., 42 pls. £7.00, hard-back.

This survey was prepared by Dr I. F. Smith, Dr B. N. Eagles and H. C. Bowen of the Royal Commission's Salisbury Office. It continues the high academic standards of research and fieldwork that are the hallmark of that body. The standard of presentation, lavishness of illustration, fine paper and binding are of a quality that must excite both admiration and envy.

Forty-two long barrows in the two counties are described; some massive upstanding field monuments, but others plough degraded or only visible as crop marks. Each site has a detailed descriptive entry that considers location, present day condition, history and associated finds. The accompanying plans are contoured or hatched. Many include the results of geophysical surveys specially undertaken for this project. Ground level photographs illustrate the upstanding monuments with an artist's view supplementing the description of the Duck's Nest Longbarrow at Rockbourne. However, it is the air photographs, both vertical and oblique views that will be to many the outstanding body of information provided in this work. The air photographs also demonstrate the changes wrought in the landscape during the present century. This is well demonstrated on the views of the Danebury group, of three long and one round barrow, photographed in 1924 and again recently (pls. 24 and 25).

The investigators have drawn on a wide range of sources for their air photographic evidence and made good use of this in interpreting sites. This extends to supposed long barrows such as Dawn Grange and Freefolk Wood, sites that were readily seen from the air to be instances of several confluent circular mounds (pls. 2 and 4). It would have been useful for the interpretation of photographs if the date and crop conditions were available in every caption. It is not clear if the long barrows in pls. 27, 35 and 41 appear as crop or soil marks. Also many aerial archaeologists will be puzzled by the light coloured marks, indicating ditches, being described as 'parch marks', in the captions of pls. 26 and 37. These are more likely to be crop reversal marks where the lush growth on the deeper soil is denser and brighter in colour and contrast to the thinner growth round about. Certainly the date and ground condition data for these photographs are required to resolve this problem.

The Hampshire sites are an eastward extension of the great concentration of Long Barrows on the Wessex chalk of Wiltshire and Dorset. Their apparent scarcity is the product of Roman and medieval cultivation, the same fate befell others in similar highly cultivated regions of England. Only two Hampshire long barrows have been excavated to modern standards, Holdenhurst and Nutbane. Holdenhurst, on gravel, had its mound enclosed within a U-shaped quarry ditch, an arrangement recognisable again at the two Whitbury barrows. Nutbane, on chalk, had a pair of flanking side ditches, the usual arrangement in the Hampshire long barrows. This class can be divided into a lesser subgroup with short, broad mounds such as Bramdean and Rockbourne. More numerous are the elongated mounds, some with wide berms between mound and ditches seen at Nutbane and Whitchurch. A disappointing feature of the air cover is the lack of internal features such as enclosures and facades appearing as crop marks on extensively levelled sites. Such marks can be seen at Nutbane (pl. 31) where the bedding trenches of the excavated forecourt buildings have created destructive growth.

It is a minor irritation that a county numbering has not been used for the sites; it would have been an aid to reference in discussion and in the comparative plans (Figs. 2 and 3).

T. G. MANBY

BOOK REVIEWS

MERCER, R. J. and SAVILLE, A. *Grimes Graves, Norfolk, excavations 1971–72*. Vol. 1 by R. J. Mercer. Vol. 2: *The flint assemblage* by A. Saville (Department of the Environment Archaeological Reports 11). London, HMSO, 1981. vii + 122 pp., 59 figs., 25 tables, £14.00; ix + 182 pp., 106 figs. £19.00

This is a masterly account of an excavation which made exceptional demands on human ingenuity and endurance, involving the extraction of more than 450 m³ of fill from a 10 m deep flint mine shaft and its galleries over a period of five months, together with the less arduous excavation of the surrounding surface area.

Mercer's account of the excavation is lucid, logical and readable. An unfashionable alternation between description and discussion is handled with such skill that it is illuminating rather than confusing. Standards of excavation, observation and recording far higher than those previously attained on the site have led to the recovery of detailed information about the method, scale and timing of the original working of the excavated shaft, which has in turn made possible a convincing and practical work study. Pottery finds and radiocarbon dates show that the shaft was worked by users of grooved ware in the early second millennium bc. Previously ill-understood discoveries at Grime's Graves are put into perspective: human remains found during the 1914 excavation of pits 1 and 2 may perhaps be contemporary with Iron Age burials found near the top of the 1971 shaft, while later Bronze Age débris found in several earlier excavations is now shown to derive from an extensive re-occupation of the site in the late second millennium bc, some 500 years after the mining period. The elucidation of this phase is of particular interest, since the featureless rubbish deposits which seem to mark contemporary living sites in East Anglia survive only in exceptional circumstances, whether tipped into the tops of old mine shafts, as here, or covered by peat, as on the fen edge to the west.

A major strength of the publication is its specialist reports, by I. H. Longworth on the Neolithic and Bronze Age pottery, by R. P. Kenward on the human skeletal material, by J. G. Evans and H. Jones on the sub-fossil land-snail faunas from Grime's Graves and other Neolithic flint mines, by A. J. Legge on the agricultural economy, and, in vol 2, by A. Saville on the flint assemblage. The last two may be singled out for comment. Legge combines faunal, botanical and pedological evidence to achieve an impressive reconstruction of the mixed arable and pastoral later Bronze Age economy, in which dairying seems to have been more important than meat production. Saville's report on the 400,000–500,000 pieces of struck flint is perhaps as great a feat of endurance as the excavation itself. This is the first time that flint assemblages from Grime's Graves have been described and assessed *in toto* rather than selectively, with a corresponding increase in the understanding of the techniques and products of the mining period and of the nature of contemporary activity on the site. His comprehensive recording of the material provides a basis for the comparative study of assemblages from this and other industrial sites. His account of the later Bronze Age industry brings out both its technological relation to those of the mining period and its functional divergence from them, emphasising the different character of the two main phases of prehistoric activity on the site.

General commendation must be tempered with specific reservations. Most flint artefacts are illustrated at 2:3, with the largest pieces at 1:3, although a uniform scale of 1:2 would have been adequate, cheaper, and much less confusing to the eye. Saville's report was finished in 1974, and a substantial part of it consists of the presentation and discussion of comparative material which has been superseded by subsequent syntheses, especially Pitts' work on flake proportions and Green's on flint arrowheads. His presentation tends to indigestibility, which often obscures the value of the content. The initial typological definitions might have been clarified by consistent reference to appropriate illustrations, as might some other parts of the text. Some tool types are partly defined by their macroscopically recognised utilisation (vol. 2, pp. 10–11), soon after it is stated that 'reliable criteria for the macroscopic recognition of utilisation have yet to be established' (vol. 2, p. 7).

In the publication as a whole, plans and sections would be easier to 'read' if their conventions were more representational and less diagrammatic. Individual sections and specialist reports might be better co-ordinated: an admirable attempt is made to link later Bronze Age economic evidence and artefactual repertoire (vol. 1, p. 38), but discrepancies occur elsewhere. Nearby metalworking is declared unlikely (vol. 1, p. 74), but is affirmed in another passage, apparently written later, in the light of further information (vol. 1, p. 36). Legge describes the dumping of chalk spoil from the mines on the sandy soil of the site as creating 'a large area of artificially improved pasture' (vol. 1, p. 96),

BOOK REVIEWS

while Mercer describes the same circumstance as 'favourable to arable farming' (vol. 1, p. 38), disregarding the perhaps incultivable irregularity of the dump-covered surface. Saville's reluctance to see Grime's Graves as an axe-factory because of the low proportion of finished and unfinished axes found in 1971-2 (vol. 2, pp. 55, 69-72) contrasts with Mercer's view that heavy edge tools were a major product of the mining phase but were finished elsewhere (vol. 1, p. 35).

Evidence not considered in the report tends to support the second view: preliminary results of the British Museum's flint analysis programme suggest that flint axes from provenances near Grime's Graves may be made of flint mined there; and flake axes of triangular outline and plano-convex section, like some demonstrably made on the site (vol. 2, pp. 51-5), are, in the writer's experience, often found in the extensive surface collections from the surrounding Breckland. Exceptionally low proportions of cores and implements in mining period contexts (vol. 2, p. 37), would be consistent with the removal from the site of axes and dressed nodules. Local dispersal of the latter may be indicated by the presence in second millennium bc industries from other parts of south-west Norfolk of fresh black flint for which there is no immediate source. In other respects, too, the results of the excavation are presented in a vacuum. No team coming into an area to conduct an excavation can achieve instant local knowledge, but it is none the less regrettable that the site is discussed in the context of Sussex and Wessex flint mines rather than of local settlement and economy. There might also have been fuller communication between the 1971-2 team and members of the British Museum's 1972-6 Grime's Graves research programme on subjects like the extent of the site and the dating of the 'primitive' pits.

None of these minor shortcomings detracts from the overall excellence of the publication, which makes a valuable contribution to the understanding of both Neolithic mining and flint-working and Bronze Age settlement, at Grimes Graves and beyond.

FRANCES HEALY

GUILBERT, G. (ed.). *Hill-fort Studies. Essays in honour of A. H. A. Hogg*. Leicester, Leicester University Press, 1981. 216 pp., illus. £19.00.

This slim volume in honour of one of Wales' great modern field workers consists of a varied collection of essays concerned with hill-forts. Coverage might be considered patchy but the contributions are of particular areas and problems in detail rather than being general summaries. They reflect the great time range over which hill-forts were in use, and the many ways in which surface and excavated data can be used.

Three papers are concerned with the functions of hill-forts. Richard Bradley considers the relationship between hill-forts and earlier ritual monuments. Whereas some previous scholars interested in this relationship have seen the hill-forts as continuing the functions of causewayed camps or barrows, Bradley suggests that the superimposition of hill-forts on such sites indicates not continuity but the opposite, that the sites had lost their earlier sanctity. Some of the figures produced relating to general Iron Age settlement and earlier ritual and funerary sites seem to support this, but are not necessarily to be applied everywhere. Evidence from Ireland (deliberately excluded from Bradley's paper) suggests positioning of hill-forts on certain earlier monuments associated with cult figures particularly important to the social group. This may well have also been the case in Britain. Nevertheless, it is useful that the assumption that superimposition means continuity in religious significance has been challenged. John Collis examines the problem of hill-fort distributions, and what changes in settlement pattern (and so in society and economy) this might represent. The actual uses of hill-forts are likely to have changed through time, and the assumption that they acted as central places is challenged. This assumption has yet to be effectively proven even on sites with extensive excavation, as the dependence of subsidiary settlements on the hill-fort has yet to be demonstrated. Graeme Guilbert concerns himself with the problems of previous ambitious attempts at population estimates and the recognition of functions of structures. Clearly, what is needed is the location and excavation of sites with better stratigraphy to allow some more definite recognition of function through the presence or absence of hearths, and the distribution of artefacts and faunal evidence within and between structures. Only when habitations as opposed to mere buildings can be recognised can any attempt at more accurate population figures be made.

Interpretation from surface survey or from limited excavation is the intention of Ian Burrow and Ian Ralston in their papers. Burrow's study of Somerset hill-forts and the efforts at recognition of post-Roman occupation or alteration indicates how elusive such evidence is, and one is left, as is so often the case with Dark Age studies, with a sense of frustration rather than the satisfaction of any definite result. Ralston, on the other hand, indicates how the use of timber in the construction of ramparts can be assumed even on surface evidence such as the presence of characteristic nails or of calcined material. Even combined with the generally old and scrappy excavation data, some useful conclusions are drawn, and the distribution maps and catalogue (no doubt incomplete but still invaluable) will be useful to many other workers. The post-Roman use of hill-forts is further emphasised by the final paper by Leslie Alcock, where the documentary evidence and selective excavation has produced, in contrast to Burrow's work, some positive results. Again, a catalogue of sites is given.

One paper, the longest in the volume, considers excavated material in its own right. Michael Avery suggests a new scheme of dating furrowed bowls and carinated Hawkes A pottery, using Continental parallels but supported by the C-14 dates that had, till now, floated separately from the ceramic chronologies. The implications of this paper, if accepted, will filter through the whole of south-eastern Iron Age studies and may lead to some significant reassessments.

The production of the volume is to a high quality, and all the illustrations have been drawn in a standard style, using the same lettering (Futura medium, the reviewer's own favourite). The bibliography is combined at the end of the volume, which avoids the tedious repetition of the same references at the end of each chapter. Although it seems expensive, there is a fund of valuable material in this book, with a delightful tribute to A. H. A. Hogg by Stephen Briggs to remind us that hill-forts were built by, and now studied by, real people.

HAROLD MYTUM

BURGESS, C. *The age of Stonehenge* (History in the Landscape Series). London, Dent, 1980. 402 pp., illus. £12.00.

The age of Stonehenge is an unfortunately popular sounding title to a very detailed and serious book. A casual visitor to Stonehenge would not find it particularly useful. Students of the third and second millennia certainly would. The contents list sets the tone of the book. Gone are the Neolithic and the Bronze Ages, and instead we see the Prehistory of the Third Millennium and the Prehistory of the Second Millennium. The emphasis is on continuity and development rather than sudden and all-embracing points of change.

The two general chapters are followed by four chapters covering such themes as social organisation, settlements, crafts and burial. All contain great detail about sites long forgotten or recently excavated together with the well-known sites. The mass of detail is invaluable to any student of the period. At first sight the book appears to be the student textbook we have been waiting for. There are, however, problems.

Having got rid of the Late Neolithic and the Early Bronze Age we are instead presented with the Meldon Bridge, Mount Pleasant, Overton, Bedd Branwen and Knighton Heath Periods. Fair enough if well defined but, unfortunately, the 'Bronze Age' reappears on page 23, while by page 119 the inverted commas have gone. Even worse, the 'Wessex Culture' reappears on page 99, losing its inverted commas almost at once. We are left with 'Wessex' chiefdoms of the 'Wessex Culture' of the Overton Period of the 'Bronze Age'.

Even greater problems arise when trying to use this book as a reference book. There are no references in the text. References to individual sites may be found by looking them up in the site list at the back of the book. Without references in the text it is, however, impossible to follow up any ideas mentioned.

The book is well illustrated although some of the, otherwise excellent, reconstructions have not reproduced well. This is clearly a notable attempt to escape from the 'Three Ages' and as such is essential reading for students of the period.

PETER DREWETT

CLARKE, Giles. *Pre-Roman and Roman Winchester; Part II. The Roman cemetery at Lankhills* (Winchester Studies 3). Oxford, Clarendon Press, 1979. xli + 468 pp., 105 figs., 17 pls. £40.00.

This is perhaps the most exhaustive cemetery report yet published in Britain. The story of the discovery of the Roman cemetery outside the north gate of Winchester and its subsequent excavation is an object lesson in the importance of the amateur in archaeology, and the vital necessity of drive and initiative in any project which is to come to completion. The initial excavation was begun by a schoolboy who, admittedly, had been trained in the Winchester tradition of excavation, and the work was seen through to completion by that one person, Giles Clarke, though of course his debt to the facilities of the Winchester Research Unit is obvious. What I wish to emphasise is the point that this volume, part of which has already been judged worthy of a London PhD, has depended throughout on the enthusiasm and brain of someone who, although a student of archaeology, has never been a professional archaeologist.

In 455 pages some 451 burials are described, commented upon and analysed. This may seem a high ration of pages to burials, and yet most of the information about each burial is contained in grave plans and tables, so that most of the text is taken up with comment and analysis. Most of the burials are inhumations, so the seven cremations form a minor part of the report. Nevertheless it is quite plain that this rite is a continuing part of burial practice in Britain in the 3rd and 4th centuries, and the presence of cremations in the 4th century is something to be expected in any large cemetery.

The inhumations produced a large number of grave-goods and attributes; all were meticulously recorded in excavation and are presented for the reader. Whatever comments may be made about the interpretation of this site, it is most unlikely that the process of recording the basic data and presenting the records to the reader will be faulted in the near future. Thus the graves themselves have depths, shapes, and sides which are more or less well trimmed and more or less rectangular; all this is presented and used for informed and informing comment. The grave-goods are all dealt with as individual objects, class by class, coins, by bracelets, by glass vessels, but they are also considered as groups of objects occurring in graves in different permutations and combinations. Cross-bow brooches and belt buckles form a particularly important group of finds; as classes of objects Clarke dissects them out and improves on previously accepted typologies; as groups of finds inside individual graves he is able to point out a method of grave furnishing which cannot be paralleled in Britain, and seems most at home in the Pannonian provinces of the Empire. These graves, with belts and brooches form one of the groups of people that Clarke suggests arrived in Britain during the 4th century, made themselves at home, and then slowly melted into the Romano-British background by adopting local burial customs and grave-goods. A second class of new arrivals who form a much more disparate group are labelled by Clarke 'Anglo-Saxons' a term guaranteed to bring down on him disbelief and doubt from most lovers of Dark Age metalwork.

The chronology of the graves as finally expressed is complex and cannot be grasped by skimming quickly through the text; it needs good solid study. I think that it is in the last inspection secure, so that many graves are dated to a period of 30 years or so with only a minor period of likely error. The report is well provided with specialist reports on coins, pottery, glass vessels, glass beads, combs, textiles and chicken bones. But the bodies are not there.

It should be clear by this point that I have the highest admiration for this report, but in this one respect it fails; there is no detail of the bones found in the several graves. This means that the best cemetery report available is also one which is incomplete, and we still have to wait for a study which integrates the people who were buried, with the graves that they were buried in, the things that they were wearing, and the religion that they might have practised. The factors which lead to this failure cannot be discussed in a review, and they are more or less beyond the control of the author, but there is at present a strong possibility that these bones may never be published, and that we may never know who the people in the graves were in any human terms. I very much hope that my fears are unfounded.

The novelty in this report is the fullness of publication and the lengths to which analysis have been carried. All future reports must adopt these standards or be judged as failures. This, with one major blemish, is a triumphant success.

RICHARD REECE

BOOK REVIEWS

NEAL, David S. *Roman mosaics in Britain* (Britannia Monograph Series 1). London, Society for the Promotion of Roman Studies, 1981. 127 pp., 26 figs., 88 pls., 2 fiches of col. pls. £8.25.

The boom in rescue archaeology in the 1960s and 1970s lead to the stockpiling of a large amount of information on sites, buildings, structures, furnishings and finds. As usual the disease produced its own remedy, and groups of specialists have grown up to deal with the material. One specialist who has set his own extremely high standards is David Neal, artist by training, archaeologist by inclination and profession, who chose to take on the recording of mosaics. A second product of the boom is the new series of monographs at county and national level which are growing up to publish the material. This volume at once launches a whole collection of David Neal's mosaic drawings, and the monograph series associated with the journal *Britannia*. The standards of the monograph equal the standards of the artist so that we see a remarkable volume, of considerable use and value, at a price of less than £10.

The original drawings were almost all done by Neal as the mosaics were discovered, at a uniform scale, and with uniform conventions. They were then coloured in with the help of notes and pastel crayons, and the final version was painted in water colours at a later date. Eighty-eight mosaics, or fragments of mosaics are illustrated in black and white plates, six are also printed in full colour, and the original colour paintings are all reproduced on two fiches in pockets at the end of the volume. To accompany the illustrations Neal has given us notes on each pavement which summarise what is known of affinities, style, workmanship, layout, materials and date. The result is a superb handlist which brings out so many interesting points for the first time that an immediate reaction on the part of most readers must surely be to wish for a Neal-edited corpus of mosaics in Britain.

The author quotes the work of David Smith on mosaics in Britain extensively, and quite rightly, but he is not afraid to suggest how this work may be developed, and modified, and improved. It is good to record that no one will appreciate this more, I suspect, than David Smith himself. The very act of bringing these drawings together starts all sorts of new thoughts, especially on the motif of interlocking squares in the south and west of Britain, perhaps related to the Corinthian schools, and perhaps forming a link between the earlier school and the later.

Production and reproduction are excellent, the format is ideal and, in desperation for a criticism, I can only record that the micro fiches tend to fall out of their pockets if you wave the volume about upside down.

RICHARD REECE

BIRLEY, A. *The people of Roman Britain*. London, Batsford, 1979. 224 pp., illus. £8.95.

The immediately striking aspect of this book is the exuberant scholarship of Professor Birley. His enthusiasm for the personal details, the social background, even the names of individuals living or working in Roman Britain is unbounded: the book is, in effect, a celebration of those people whose memorials, both literary and epigraphic, have managed to survive the centuries down to recent times. As such, it is a fascinating compilation, for in 16 chapters, the author has mentioned virtually everybody whose name we know of connected with Roman Britain. Some have a surprising amount known about them, for instance the legionary centurion, Petronius Fortunatus, who served 50 years in the army, 46 of them as a centurion in 13 different legions, including VI Victrix at York. This we know from an inscription he set up in North Africa, and it is one of the singular advantages of the book that it has nearly all of the continental inscriptions that refer to Britons or to people serving or working in Britain.

Many of the people mentioned, however, are names only, although it must be said that the complexity of full Roman nomenclature renders the different parts of the name very useful for analysis of area of origin, social class, family ties and so on. This means that many of the names *per se* are of great interest. Having said this, though, I cannot help thinking that many of the interesting facts to be gleaned from this book are just names and places but no more. Little, for instance, can be said about Helenus, a goldsmith who inscribed his name on an object from Birrens (p. 130) except that he existed. Nothing can be deduced about the goldsmithing trade, the wealth of the

frontier garrisons or a number of other pertinent social and economic questions without amassing much more data than is contained in the bald statement of Birley's that attests his existence. This is one of the faults of the book, that in places it sacrifices the wider significance of the people and their doings to the desire to include more facts about others. As a result, the book can become rather indigestible, abounding in particularisms. It would not appeal to the processual archaeologist or historian, not only because of the mass of individual details, but also because the basic source material, inscriptions, is inherently and indefinitely biased in favour of literate, relatively rich or skilled persons. The analyst of social structure would baulk at the task of providing such an analysis for Roman Britain if confronted solely by the literary and epigraphic evidence contained in this book. Nevertheless, it is a book which I am sure will become essential 'second-stage' reading for students of Roman Britain.

ANTHONY KING

ROWLEY, R. T. *The origins of open field agriculture*. London, Croom Helm, 1980. 258 pp., 30 illus. £14.95.

DODGSHON, R. A. *The origin of British field systems: an interpretation*. London, Academic Press, 1980. xiv + 165 pp., 18 illus. £10.40.

I suppose the use of the word 'origins' in any book title invites criticism for it means different things to different people, and two reviewers are seldom going to agree just how far back you have to go to get to 'origins'. I offered to review these two books because of an interest in field patterns and hedges and in hopes of reading a description of *what* the English open field system grew out of, rather than *how* it evolved. I admit to disappointment in that neither the articles of the one book, nor the narrative of the other really get down to basic exploration of what the possibilities are for the organisation of agriculture before about AD 700. I am not complaining of lack of facts, that is taken for granted, for they just do not exist; my complaint is that while several of the authors involved, notably Dodgshon in his book and the article in the collected papers, and Campbell and Fox, all consider different ideas and models of the development of the medieval field system, none of them actually tries out different models of the possible earlier field structures.

This is one more example of the absolutely natural and understandable concentration of work on what we know something about, rather than what we know nothing about. The former is obviously more easily planned, and formulated, as in the examples of Hooke, in the volume of papers, who looks at the evidence in the early charters of the West Midlands, or Roberts, who examines the case of the Durham village of Cockfield, and the beginnings of its Townfields. But it is the latter, which is much more difficult to enforce as a subject of writing or discussion (how can I contribute a paper on something we know nothing about?) which, by definition, is both more interesting and in need of study.

Two papers in the collection deal with material study, one by Taylor the general archaeological evidence, and one by Hall on the field-work evidence for the shape and size of fields and strips. Both are useful summaries which can range more widely than the documentary contributions because they have an unlimited date range. In practice they are both confounded by our astonishing lack of knowledge of Roman field systems. Here is a further point at which models would have been useful, even if they were based on no evidence at all – just free-floating ideas. It is one thing, and a rather hopeless thing, to say to archaeologists, 'We want to know about Roman field systems', but a totally different thing to say 'I think Roman fields were like this – when you find a ditch or field boundary it should be cut in this way if my ideas are right'. In the first case you are asking someone versed in stones and mortar, who is normally petrified in the presence of ideas, to go out, without any further help, and think constructively about Roman fields; the outlook is bleak. In the second case you can tell him what he ought to find if you are right, and what he might find if someone else is right, and the digger will then greatly enjoy coming back to you and saying 'You were wrong – it looked like this'. As a result one model is less likely than another, and at least the illusion of progress is maintained.

BOOK REVIEWS

These two books therefore seem to be an excellent introduction to the development of the English Medieval field systems, from their beginnings to the high Middle Ages, the one an individual view, which naturally reads more smoothly and has less gaps, the other – a composite account from several different points of view. But neither tackles the question of the *origins* of field systems, at least as I understand it.

RICHARD REECE

FENTON, Alexander. *The Northern Isles: Orkney and Shetland*. Edinburgh, John Donald, 1978. x + 721 pp., 287 illus. £15.00.

The author describes the Northern Isles as an amalgam of land and sea, fields and houses, boats and cliffs. His approach has been 'to try to show the details of the amalgam, how the elements interlock in different ways and degrees' (p.v) through an examination of the island communities in relation to their organisation of the resources of the sea and of the land for subsistence, rent and commerce. Perhaps inevitably in a pioneering work documentation rather than critical examination or hypothesis is the mainstay of the study. The author has assembled a mass of information which is of great potential use to archaeologists, not for what it says about the archaeology of the Northern Isles (the author's perspective generally reaches only as far back as the Norse impact) but rather as a means of expanding our ability to interpret features of simple technology and subsistence when they occur as part of archaeological evidence. The documentation in a straightforward, almost telegraphic, narrative of ecologically well adapted processes (from line fishing to ploughing, rope making to roofing, manuring to peat cutting, spade cultivation to threshing, grain drying to grinding, stock husbandry to milk production, wild fowling, to whale hunting, to pig killing) lends insights to many features of settlement archaeology.

A few examples of such illuminations, taken at random, must suffice for many. The continual adaptation and reuse of buildings seen on any Orkney farm warns against the dangers of ascribing, Pompeii-like, one particular function to an excavated building on the basis of its final condition. A more specific example is the pit beside the hearth which is a feature of many excavated British pre-historic houses: in Orcadian houses, such a feature was used as an ash pit (and hence a source of preservative). The value of cow dung as fuel on generally treeless islands is demonstrated to the extent that different types of dung were distinguished and collected. Large mallets were used for clod breaking, while fish were attracted to baited lines by the spitting of chewed limpets on to the sea. Again more generally the use of sandy coastal areas for early agriculture is now recognised: the importance of fertilising such areas is demonstrated by the Orcadian decision to cultivate only those areas for which seaweed was readily available.

As to be expected from a former member of the Dictionary of the Older Scottish Tongue, the etymology of the many place names and local words is clearly and fully explained. However the use of archaeological data is less surefooted, e.g. allusion to Mesopotamian parallels for herringbone walling of chambered tombs, or the omission, in the section on the use of peat in house walls, of mention of its use as an important element in the construction of the Shetland houses of the second millennium bc. This is an unfortunate defect, particularly as in Orkney, at least, the inhabitants identify very closely with the past and demonstrate an almost competitive pride in their local monuments and excavations.

On the technical side it is a pity that some of the abundant and pertinent illustrations have been over reduced and that the reader is not referred from the text to the illustrations, although the latter are generally well placed throughout the book.

Contrary to the claims of the introduction, the concluding section offers no synthesis of the great quantity of data presented in the preceding 600+ pages. However, if as archaeologists we can agree with Ascher that the observational fields of ethnology and archaeology overlap on that proportion of a living community which is in the process of transformation (*Southwestern J Anthropol*, 17(4), 1961, 317–25) then we will be content to thank Mr Fenton for providing a rich seam of ethnological data which will expand the always hazardous process of analogy.

IAN A. G. SHEPHERD

BOOK REVIEWS

THE ROYAL COMMISSION ON THE ANCIENT AND HISTORICAL MONUMENTS OF SCOTLAND. *Argyll, an inventory of the monuments*. Vol. 3. *Mull, Tiree, Coll and Northern Argyll (excluding the early medieval and later monuments of Iona)*. Edinburgh, RCAHMS, 1980. xlvii + 280 pp., 262 figs., 109 pls., 1 loose map. £52.00.

The latest substantial volume from the RCAHMS is of similar quality and style to the previous ones, and again covers an area far richer in archaeological remains than the scanty research in recent times would suggest. It is a pity to see, therefore, the lack of initiative shown in what can really be considered as a glorified catalogue of sites. There is almost no synthesis, and that which is presented at the beginning is almost an insult, dealing with no problems of the data, or making any suggestions about how further work could exploit what has been so laboriously compiled. This is not the fault of those who work for the Commission but a product of the system itself. As a result we have a volume suitable only for institutions to purchase, and are deprived of the opinions of those skilled fieldworkers who have studied this area for a number of years.

The most obvious defect when dealing with the prehistoric periods is the very biased nature of the evidence. The material from the sandhills indicates that open sites were present, and no doubt not only on the coast, and even the limited excavations on the sandhills indicate the presence of wooden houses. This is itself instructive, and makes the recognition of unenclosed settlements even more difficult. For the Bronze Age, the presence of clearance cairns might suggest where to look, but by the Iron Age we only have the forts and duns, themselves of great interest, but only part of the picture.

The Early Christian period is wholly conceived in terms of the ecclesiastical monuments, of which there are few since the rich treasure-house of Iona is a subject of a separate volume. There must have been a continuing secular presence, but that does not reappear until the full medieval period when we have castles and crannogs. It is interesting to note here that the crannogs in this part of the world all show medieval occupation, and although some may have been used earlier, there are no obvious Iron Age examples as there are to the south. The castles did attract some concentrations of settlement, as can be seen clearly at Ardtorninish (p. 170) and Aros (p. 173). The hall-houses with dependent buildings beyond appear more as the northern version of a moated site and village than a really heavily fortified site, although some of the castles are more substantial. Other medieval settlements may underlie the later townships such as Bourblaige (p. 238) and Invea (p. 242). The inclusion of a few later sites, both domestic and industrial, adds to the value of the volume, and the lighthouses of the last century are also described fully.

In the environment of the west of Scotland, transhumance must have been an important economic strategy throughout prehistory and indeed continued until recently. There is little mention of this in the volume till the late medieval shielings, including large concentrations as at Core Bhorradail (p. 240), except for the now-traditional lip-service to the concept of the mesolithic seasonally occupied site. We have now reached the point where mesolithic must equal transhumant, without any real evidence necessary to support this. The medieval example is probably of relevance to the Iron Age, and possibly earlier, and upland summer grazing was almost certainly carried out to increase the carrying capacity of this beautiful but not always generous land.

This review has concentrated on some of the disadvantages with the Royal Commission volumes, criticisms that are applicable not only to this one. There are many points in their favour. The sites are lucidly described, and the plans and photographs are of good quality. The book is well printed and bound, but costs a gargantuan £52, in contrast to *Argyll 2* which cost a mere £17.50. What will we have to pay for *Argyll 4*?

HAROLD MYTUM

DAMS, Lya. *L'art pariétal de la grotte du Roc Saint-Cirq*. (BAR Int. Ser. 79). Oxford, British Archaeological Reports, 1980. 149 pp., 52 figs., 38 pls. £9.00.

This monograph on the art in the cave of Roc Saint-Cirq (also known as the Noel Brousse Cave), near Les Eyzies, Dordogne, France, is a welcome addition to the published corpus of Palaeolithic cave art. Much new information is presented here by Lya Dams.

Engravings of 38 animals (27 horse, three bison, two caprid, one possible bird, and five animals of indeterminate species), two humans, and 18 signs are succinctly described in word, drawing and,

sometimes, black and white photography, together with a generous number of pull-out plans of the cave and the spatial relationships of the art.

Although not one of the *géants* of cave art, known mainly for its engraving of an ithyphallic human figure, Saint-Cirq is interesting not least because excavations (from 1918 and later) in the porch and front gallery of the cave have revealed archaeological layers dating to Magdalenian II and III. Dams tries to relate the wall engravings to these reference points and to stylistic criteria derived from other decorated caves, coming up with three possible stages of decoration for Saint-Cirq ranging from the Solutrean–Early Magdalenian to the Final Magdalenian.

In an interesting brief conclusion, Dams rejects the notion that there could have been year-round habitation by the people associated with all the many decorated caves and archaeological sites known from the Late Magdalenian in the Les Eyzies area: the animal resources would not have been sufficient. Instead, she suggests that while following the herds on their seasonal migrations, the peoples may have treated the region as a temporary stop of great cultural significance, the nature of which is unknown to us but decoration of caves such as Saint-Cirq may have been part.

It is a pity that Dams, who takes her comparative data from near and far (e.g. La Pileta, southern Spain), chooses not to refer to the art (particularly, a comparable ithyphallic figure) in the nearby cave of Sous-Grand-Lac (see Delluc, B. and G.).

Within the limits of the BAR series, this monograph is a good production. The text is in French with a summary in English (which, by surprise, contains information not found in the main body of the text). A little more attention at the editing stage would have prevented some of the printing errors and some irritating small discrepancies in the numerical lists. Also, it would not take much to improve some of the plans, which are nevertheless useful. While the line drawings are perfectly good (clearer in fact than the engravings *in situ*), the photographs of the art, as so often in the publications, are difficult to read: tracing-paper overlays, as used for the BAR monograph on the art at Gargas (see Barrière), could have been put to good use here.

Finally, Dams predicts that more art may exist within the yet to be excavated and unrecorded areas of the Saint-Cirq cave, so we may hope for a complementary report later.

References

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| Barrière, Claude | 1976 | <i>L'art pariétal de la grotte de Gargas</i> , parts i and ii. Oxford: British Archaeological Reports. |
| Delluc, Brigitte and Gilles | 1971 | La grotte ornée de Sous-Grand-Lac (Dordogne) <i>Gallia Préhistoire</i> , XIV(2): 245–52. |

ALEX HOOPER

ROZOY, J. -G. *Les derniers chasseurs: l'épipaléolithique en France et en Belgique: essai de synthèse* (Bulletin de la Société Archéologique Chapeoise, Numéro Spécial, Juin 1978). 3 vols. Charleville, The author, 1978. 1256 pp., 259 pls., 294 figs. Fr.340.00.

Since this voluminous work appeared in 1978 it has not noticeably influenced relevant studies. This reviewer proposes to examine some of the reasons why this is the case.

The study comprises three volumes, including 1193 pages of text and more than 500 illustrations, graphs and maps. Though it is written and produced with clarity, and it contains summaries in English and German at the end of each chapter, its sheer size is daunting. Written by one man, over what was probably a long period of time, the study is full of contradictions and, in the final analysis, it is simply too large for him to maintain control over the material.

Rozoy states that the object of the study is the Epi-Palaeolithic period, defined by the massive introduction of microlithic 'armatures' (p. 17) and ending with food production. The definition precludes the idea of 'Mesolithic'. Rozoy goes on to say that the work is not just a history of research into the Epi-Palaeolithic but also a critical analysis of the scientific thought brought to bear on it (p. 19). Rozoy claims that the study is not exhaustive, but he calls it a synthesis (p. 17).

When Rozoy diverges from description and makes generalisations (e.g. p. 128, where he argues against giving tool types functional attributes), his discussions are soundly based and full of common

BOOK REVIEWS

sense. Yet, his choice of material belies his sound judgement. To see the final Palaeolithic and Mesolithic as a continuum is commendable. Even so, Rozoy's definition of Epi-Palaeolithic not only emphasises abrupt technological change but includes the word 'armatures', which Rozoy clearly identifies with projectile points (p. 949). By identifying form with function on a grand scale and without very much evidence, his Epi-Palaeolithic is quite arbitrary, excluding, for example, the final Magdalenian with backed bladelets (p. 17).

Because Rozoy does, in fact, include some discussion of the final Magdalenian and Azilian (e.g. La Gare de Couze and La Tourasse), his 'history of research into the Epi-Palaeolithic' is hardly complete without some discussion of important sites, under research prior to 1978, such as Pincevent, Etiolles, Le Faurelie II, Bois Ragot, Pont d'Ambon, Meer.

If syntheses exist to bring order out of chaos (see the Preface), then all of the chaos should have been considered, not just some of it. Rozoy divides France and Belgium into arbitrary regions, e.g. south-west France. In some regions he has recently excavated material at his disposal and in others only surface finds. More than half of Rozoy's study is devoted to the exposition of flint tool assemblages from selected sites, but because of the variety of contexts represented and the existence of reliable material which is not mentioned, Rozoy's sample selection remains mysterious. His inter-site comparisons are based almost solely upon cumulative graphs which, in turn, are based upon Rozoy's own typology, which is bulky. The type list includes too much under the guise of morphology. The flint analysis would be better served by separate investigations into size, technology and function.

The second part of volume two, as an excursion into Epi-Palaeolithic way of life, is more interesting and better executed than all of the rest. Here, Rozoy discusses habitat, man-animal relationships, tool functions, burial, art and ritual. This section is truly a synthesis, and Rozoy would have been more just to himself to have published it separately. The bibliography alone is an invaluable asset to students of prehistory for this period.

Taken as a whole *Les derniers chasseurs* attempts too much. It may be impossible for one man to touch so many disciplines within archaeology and to do justice to each one.

EMILY H. MOSS

HOOKER, J. T. *Mycenaean Greece*. London, Routledge and Kegan Paul, 1976. xiii + 316 pp., 13 figs. £6.50.

This book offers a historical review of the events and problems of the Aegean Bronze Age and is primarily concerned with the Greek mainland; nevertheless, developments in Crete and the Aegean islands are also discussed. The book is arranged in eight chapters. The first sets out five types of available evidence: material remains; documents in linear scripts; Greek dialects and the Homeric poems; evidence from Egypt and the Hittites, and Bronze Age survivals in Greek literature.

Next, the author re-examines the question of the origin of the Greeks, drawing on what evidence can be obtained from Greek historians as well as on the linguistic and archaeological evidence.

In the third chapter, the beginnings of the Mycenaean Age come under scrutiny – in particular, contacts with Crete and the origins and contents of the Shaft Graves.

The fourth chapter examines relations between Crete, the mainland and overseas during the early Mycenaean Age.

The mature Mycenaean Age, its culture, problems and interpretation are discussed in the fifth chapter where there is also an account of Mycenaean sites on the Greek mainland.

The sixth chapter studies the Mycenaean expansion overseas, including the Aegean, Cyprus, Anatolia and the Levant. There is a comprehensive discussion of the evidence for relations with the Hittites.

The seventh chapter concerns the end of the Mycenaean Age. The destructions that brought about the fragmentation of Mycenaean culture are discussed at some length. There is a lucid account of possible causes of destructions, as well as the role of the Sea Peoples and of the Dorians.

The final chapter deals with the economic, political and religious structure of the Mycenaean world as seen through Linear B.

BOOK REVIEWS

The book also contains an appendix of extracts from ancient texts referring to the Dorian invasion. A second appendix gives a catalogue of important Mycenaean studies. Eleven tables, two maps and eleven illustrations of material remains enhance the value of the text.

It is worth noting that the author distinguishes three phases of Minoan influence in pottery: a, MH/LH I; b, LH II/III A1; c, LH III A2/III B. This recurrent influence of Cretan ceramics is not always fully appreciated.

This is an excellent synthesis of current knowledge, well-organised, simply written and soundly presented. The author discusses the complicated problems of Aegean archaeology without polemic or prejudice. If one does not always agree on minor matters, the pros and cons of every problem and theory are lucidly stated and the good bibliography helps the reader to reach their own conclusions. The book is highly recommended, both for libraries and individual scholars.

A. KANTA

RUTTER, J. B. and Sarah H. *The transition to Mycenaean: a stratified Middle Helladic II to late Helladic IIA pottery sequence from Ayios Stephanos in Lakonia* (Monumenta Archaeologica 4). Los Angeles, California, Institute of Archaeology, University of California, 1976. ix + 71 pp., 34 figs., 17 pls. n.p.

This book is a typological study of MH II–LH II A pottery from Ayios Stephanos in Lakonia. A description of the various wares is followed by a statistical analysis of percentage composition by ware. A chapter is dedicated to recovery standards of the pottery and stratigraphy of the deposits. In the next chapter, the pottery is analysed in detail, period by period. Parallels are drawn with Kythera, Crete and the Greek mainland. These comparisons make the book invaluable to the student of Aegean archaeology.

In the conclusions which follow, the authors point out that the site provides accurate correlations of the mainland and Cretan ceramic sequences. Two sites have been found to be fully comparable: the Argos Deiras cemetery and Kythera. The 'Minoanising' ware so prominent at the site is supposed to have been made locally by resident Kytheran or Minoan potters. The authors demonstrate that Minoan influence was present in Lakonia before the earliest local LH I pottery and consider that the earliest LH I pottery of the southern Peloponnese probably predates that of the Argolid; they conclude, therefore, that Mycenaean pottery was developed in Lakonia and Messenia rather than in the Argolid.

Concordance of catalogue numbers and deposits and excavations register numbers follow.

The authors of this excellent book set a very high standard of pottery publication, presenting the whole of the material recovered and not merely a selection, as is often the case. Nothing less can be acceptable today, with the current progress of archaeological analysis and thought. The statistical analysis which the authors have provided with such care is an indispensable part of such a study. Unfortunately, such studies are not always possible because the pottery material which has survived through millennia to be found by us today is at times thrown out arbitrarily and before adequate study.

Aegean archaeology clearly needs more pottery studies of a comparable standard to that of this fine work. One would wish to see a catalogue of all illustrated sherds and more complete drawings than those provided; but these are minor details in an admirable book of vital importance to the student of Minoan and Mycenaean ceramics of the end of the Middle and the beginning of the late Bronze Age. As such it is highly recommended.

A. KANTA

HIGGINS, Reynold. *Minoan and Mycenaean art*. Revised edition. London, Thames and Hudson, 1981. 216 pp., 241 illus. £5.95; £2.95 paperback.

This is a useful introduction to the art of ancient Crete and Mycenae and caters both for the layman and the student.

The book is divided into six chapters. After an introduction dealing with the history of scholarship, chronology and historical background, the first chapter discusses Crete from 3000 to 1550 BC.

There is a good short description of architecture, painting and pottery, faience, ivories, stone vases, plate and jewellery. Most of the views expressed are sound, representing the broad consensus of scholars and the author's familiarity with his material is generally up to date. There are, however, some lapses, for example, on pp. 34–5, in the discussion of the dubious Boston ivory and gold Snake Goddess, the author argues that 'it is accepted by most of those best qualified to judge, and we too, may accept it.' Irrespective of the figurine's authenticity, the student should beware of conclusions based on such appeals to authority rather than well-marshalled evidence and judgement. Also, on p. 13, he states that we know very little 'of the palaces . . . which were built around 2000 BC and destroyed by a terrible earthquake c. 1700 BC'. The substantial evidence from Phaistos and Zakro is strangely ignored.

The second chapter deals with the Cyclades from 3000 to 1550 BC. A short account is given of the pottery, chronology, marble statuettes and vases, the plate and the jewellery. Chapter three discusses the historical background, architecture, pottery, plate, jewellery and seals of mainland Greece from 3000 to 1500 BC.

The major arts in the Late Bronze Age are described and assessed in the fourth chapter. The historical background for Crete and the Cyclades is dealt with briefly and that for the Greek mainland at length. Architecture is presented primarily in terms of Mycenaean development, with little mention, for example, of Cretan villas and tombs. Sculpture and frescoes are also discussed. The account of the frescoes has been brought up to date, although the impressive discoveries at Thera are confined to less than half a page. They surely merited fuller treatment.

The next chapter moves to the minor arts in the Late Bronze Age. There are concise summaries of the pottery, coffins, terracotta figurines, faience, ivories, bronze statuettes, ornamental weapons, gold and silver plate, stone vases, jewellery and seals. There are again some lapses in the up-dating of the material. On p. 107 it is stated that pottery of the palace style is found nowhere else in Crete except Knossos. Palace style pottery has been known for some time from Phaistos, Hagia Triadha, Komos, Malia, Rethymnon, Chania and perhaps Chondros Viannou. On p. 122, describing motifs on Cretan sarcophagi the author considers that human figures are avoided; he ignores, thus, the evidence of important finds such as the Episkopi, Armenoi, Maroulas and Pigi sarcophagi.

The book concludes with a few notes, a small bibliography, a list of illustrations and an index.

This is a successful book within its limits and is to be recommended for libraries and individuals, now in particular, that larger picture book such the one by Marinatos and Hirmer are no longer available except in libraries.

A. KANTA

SANDARS, N. K. *The Sea Peoples: warriors of the ancient Mediterranean, 1250–1150 BC* (Ancient Peoples and Places 89). London, Thames and Hudson, 1978. 224 pp., 131 figs., 8 pls. £7.50.

STROBEL, A. *Der spätbronzezeitliche Seevölkersturm* (Beiheft zur Zeitschrift für die alttestamentliche Wissenschaft 145). Berlin, De Gruyter, 1976. xi + 201 pp. DM98.00.

It was common at one time to attribute the downfall of states and empires to invading peoples: thus the Gutians were seen as the destroyers of the Agade dynasty, the Amorites have been blamed for the downfall of the Third Dynasty of Ur and the Kassites have been assumed to hasten the disintegration of the first dynasty of Babylon. Closer examination of the foreign invaders, however, has usually resulted in demonstrating a far more complex and subtle relationship between the appearance of a new people in the documentary record and the decline of the particular state with which they were associated. In each of the cases cited above it has been demonstrated that the migrant groups were not responsible for the fall of the states they entered, but that these were already collapsing due to internal problems. Thus the so-called destructive invasions are now more often seen as disparate groups of people profiting from limited exploitation of an already unstable situation. What this view makes particularly clear is that such peoples were in many ways actually dependent on the very systems which they have been credited with destroying.

The 'sea-peoples' present yet a further group who, it has been thought, blazed a trail of destruction from Greece through Anatolia and the Levant to the borders of Egypt, thus putting an end to

the Late Bronze Age. The enormous geographical sweep of the destructions credited to them is due to the fact that their origins are unknown and their final places of settlement only partly known. They are thus a mystery people who perhaps formed a vast barbarian horde sweeping down from beyond the confines of the 'civilised' Late Bronze Age world and destroying all in their path, the analogy for which would be the Goths and the Roman Empire. While it is perfectly possible that this is what the 'sea-peoples' movement constituted, one should remember that there is no evidence to substantiate such an interpretation. Neither in Mycenaean Greece, Troy, Hittite Anatolia, Cyprus or the cities of the Levant is there either picture, artefact or text that can be said unequivocally to refer to these groups in such terms. The *only* evidence there is comes from Egypt and it is on the basis of assumptions built on these not very extensive sources, together with rather general chronological coincidence that various types of material from sites destroyed at the right period have been strained as far as possible to produce the desired answer, i.e. that the sea-peoples destroyed Mycenaean citadels, the Hittite empire, and an extensive number of Levant states including Ugarit, as well as being involved with the end of one of the Troy phases (VIA).

It is interesting that two books on this subject should have appeared within so short a time of each other, and the two very different approaches they represent are instructive in demonstrating the narrow limitations of the hard evidence and how widely interpretations based on it can differ depending on the particular interests and aims of the individual scholar.

Strobel's attitude to the problem is expressed in the title of his book which suggests a massive invading horde and is further illuminated by his introduction, in which he reveals that his interest in the subject arose from his involvement in Old Testament exegesis, particularly in relation to the history of the Israelite exodus and wanderings narratives. The book is divided into four sections each bearing a title and a suggestive subtitle: section A ('The Seapeoples' invasion in history and myth: the historical departure point') concerns primarily the archaeological and historical evidence (Egypt, Hittites, the Fall of Troy, Cyprus and the settlement of sea-peoples in Egyptian-controlled territory in Palestine); it also introduces some of the Greek legendary material deemed relevant to the sea-people problem by certain scholars, i.e. Mopsus and Teucer, as well as the less well-known story concerning the settlement of the Phoenician coast by Kasos and his troop of Argives including a band of Cretans. Section B looks at the attempts to define places of origin for the various sea-people groups; it is here that the hoary question of Keftiu/Kaphtor is discussed, together with the possibility (on the basis of later Greek tradition) of the fall of Troy resulting in the displacement of peoples; the political character of Mycenaean Greece is briefly investigated and the Ahhiyawa question again subjected to scrutiny (although the recent arguments on the re-dating of Hittite texts which has implications for this problem are not noticed); the section ends with a discussion of the possible origins of the Philistines, and an attempt at answering the question why in the 13th century BC peoples in the Aegean and Western Asia Minor (the main area of origin of the sea-peoples as he has previously concluded) uprooted themselves in order to follow an 'expansiven Drang nach Osten und Süden'. The reason for this, he concludes (following a number of earlier suggestions) was a drought which had effects of disastrous proportions – the contemporary evidence for this being Merneptah's grain gift to Hatti and the correspondence between Ugarit and Hatti concerning a consignment of grain to be sent to the Hittite realm, while a folk tradition of this disaster has been preserved in Herodotus' account of the famine in Lydia which led to an exodus by part of the population. Section C attempts the difficult and almost impossible task of defining the culture and religion of the sea-peoples after their migration ('diaspora' comes closer to Strobel's concept). In this section the possible connection of sea-peoples' groups with Etruria, Sardinia and Sicily is examined by the author and given guarded approval; the religion is examined on the basis of the Old Testament, classical writers, finds of 'cult-statues' on Cyprus and various implements interpreted as 'cult-objects' from Palestine; pottery, art, weapons, clothes, physical appearance, jewellery, language and possibly relevant text-fragments from Hebron and Der 'Allā are examined in the remainder of this section. The relatively brief section D considers the possible implications of all this gathered material for the biblical motif of 'exodus' and 'wanderings': Strobel's conclusion here is that the sea-peoples who settled in Palestine would have retained stories concerning the disaster that led to their displacement, subsequent migrations, vicissitudes and ultimate settlement and these stories fundamentally influenced the formation of a similar 'history of origins' among the Israelites. The book is completed by three extensive indexes and contains a fair number of illustrations, sketchmaps and tables throughout the text as well as extensive bibliographical footnotes.

Obviously this work is not a negligible effort and the book is useful as a guide for checking up individual items and as a handbook to the history of discussions of particular topics, but in the final analysis it is little more than that: the information is often incomplete or misleading as it is not connected with the earlier history of particular states. For example, Hittite dependance on grain imports from North Syria is well attested much earlier than the couple of Ugarit letters cited by Strobel on p. 174, as is shown by the edict concerning the merchants of Ura which dates to the reign of Hattusilis III (c. 1275–50), and there is no reason to suppose that this reflects a sudden change of policy due to climatic change or similar disturbances within Anatolia. The use made by Strobel of legends which preserve a folk memory of a specific historical event is of doubtful value: these tend to reflect far more often attempts by scholars in the Greco-Roman world to accommodate peoples and cultures they came into contact with in their own mythology by relating them either to people connected with the Trojan War and displaced as a result or to one of the many offsprings of Herakles. This makes it virtually impossible to use Greek legends as an historical source as they primarily illustrate Greek thought-processes of later periods and provide little insight into the indigenous historical traditions of non-Greeks; a fact which must surely vitiate the ultimate conclusions of Strobel's work.

Sanders, using identical evidence, has not surprisingly produced a very different book as she is primarily an archaeologist who has specialised in European prehistory. The work has been produced for the successful 'Peoples and Places' series, is therefore full of illustrations, maps, and some excellent plates and is aimed at introducing the non-specialist to a particular period of history. As she herself notes, it is something of an anomaly to produce for this specific series a study of a people who are difficult to define and even less easy to place, but she brings it off remarkably well. The first four chapters concentrate on outlining the economic, social, political and cultural situation in the eastern Mediterranean emphasising the continuity of life at subsistence level at all times, the very precarious character of the complex superstructures that developed in Anatolia, Greece and the Levant, and the fact that neither geographical factors nor climatic changes provide by themselves explanations for breakdowns or movements of peoples. Particularly valuable is the fourth chapter which concentrates on defining the culture of the Balkans, Eastern Europe and central Mediterranean area – places with which the sea-peoples have been frequently connected by those who know little about the archaeology of these regions and have thus used them as a convenient starting or finishing points for groups of people who appear suddenly out of the blue and, as mysteriously, disappear again. Having sketched the relatively stable situation of the 13th century, she moves on to examine the complete collapse of the *status quo*, resulting in the permanent disappearance of the Hittite Empire, the city-state system of the Levant and the political system of Mycenaean Greece. The book is descriptive for the most part but an interesting analysis is suggested in the final chapter: the actual collapse of the existing systems was due to inbuilt weaknesses which made them overdependent on a particular configuration of resources; little was needed to bring any one of these states to a critical point and as they were largely interrelated the disastrous fate of one would have repercussions on others. The role of the sea-peoples in this situation was *not* a causal one; rather they are to be seen as a symptom of the difficulties being experienced by the various political centres which resulted in these people extending their piratical and raiding activities as it became more difficult for the rapidly crumbling centralised states although their activities helped to worsen an already critical situation which finally led to complete collapse in most cases. With the disappearance of these particular political and economic systems the sea-peoples themselves also disappeared, i.e. the sea-peoples were a product of the systems and thus co-extensive with them.

This tentative analysis is neat and persuasive and does no violence to the sparse evidence available; whether it is correct is, of course, impossible to say but Sanders has certainly made an intriguing case for it in this well-written and not over-documented book, which does, however, suffer from occasional mistakes, e.g. the discussion on horned helmets is somewhat overlong and also wrong – horns were not worn on helmets in Mesopotamia except by gods – and Printers' errors, e.g. p. 140 where a reference is made to Rameses III's inscriptions at Karnak when surely Medinet Habu

BOOK REVIEWS

is meant. There are also a number of small points that one could take issue with such as the rather uncritical use of Papyrus Harris and the 'Israel' Stele (pp. 115–16), lack of precision of terms such as *ḥupšu* (p. 50) and Amorite (p. 45), and the map (fig. 2, pp. 14–15) which pre-empts a thorough discussion of Hittite geography and its wider implications. But these are relatively minor differences of opinion. A very valuable aspect of the book is that she has been able to include references to papers read at the Sheffield conference on the sea-peoples in 1973 which have not yet been published and is, in fact, one of the reasons for the extraordinary tardiness of this review.

AMÉLIE KUHRT

RASMUSSEN, Tom B. *Bucchero pottery from Southern Etruria*. Cambridge, Cambridge University Press, 1979. xiii + 233 pp., 65 pls. £15.00.

Etruscan studies are much neglected in Britain and it is therefore a double pleasure to welcome this book, a revised version of Rasmussen's Cambridge PhD thesis, and the first general classification of bucchero pottery from a region of Etruria to appear in print in any language.

Bucchero, the exclusively Etruscan, glossy black pottery was produced from the 7th to near the end of the 5th centuries BC. As the title indicates, this book only deals with the bucchero pottery from southern Etruria, that is the area dominated by the cities of Cerveteri, Veii and Tarquinia. The book is organised admirably: an introduction outlines the aims and difficulties involved, including those of multi-burial and violated tombs, and this is followed by a catalogue of 40 groups of pottery, selected by the author for their usefulness in establishing the typological development and chronology of the bucchero pottery forms. This selection of 40 groups of pottery from the mass of relevant but often confused material from old excavations is vital to the development of the theme of the book; it should be stressed that 33 of these groups are published here for the first time and that many of these groups were excavated by the Lerici Foundation over the last 20 years, a point fully acknowledged in the dedication. The next chapter presents a typology of 17 important bucchero forms, many sub-divided, and both the prototypes and all characteristics of these forms are well discussed and illustrated. Chapters follow on the various techniques for the decoration of bucchero pottery, and on its distribution; the conclusions include valuable general comments. There is an appendix on the inscriptions written on this pottery and a good index.

Rasmussen should be congratulated on his perseverance in tackling the vast amount of material relevant to his theme and on his sensible and reliable synthesis of the available evidence; this will be an indispensable reference book for all scholars involved in Etruscan studies for many years to come. The CUP, no doubt from economic pressures, has reproduced the text of this important book in a typewritten form, which tends to lack distinctive titles or headings; the author's fine line drawings are well printed but the reproduction of the photographs is only of a moderate quality.

ELLEN MACNARMARA

PONSICH, M. *Implantation rurale antique sur le Bas-Guadalquivir*, Vol. 2. La Campana-Palma del Río-Posadas (Publications de la Casa de Velazquez, Série Arch. 3). Paris, De Boccard, 1979. 247 pp., 85 figs., 8 pls. Pst. 2,000.

This book is the companion to *Implantation rurale* . . . Volume I (1974), which listed all of the sites found in a non-intensive survey along the banks and hinterland of the Lower Guadalquivir river between Sevilla (Roman *Hispalis*) and Santiponce (*Italica*), Alcalá del Río (*Iliipa Magna*), Lora del Río (*Axati*) and Carmona (*Carmo*) in modern Andalucía (the Roman Province of *Baetica*), southern Spain. This volume covers the area immediately up-river, between the towns of Peñafior (*Celti*) Palma del Río, and Posadas (*Detumo*), which lie to the south-west of Córdoba (*Colonia Patricia*).

For the uninitiated, the area in question was the heartland of the Baetican oil industry, and olive oil produced on estates here was sealed in the familiar globular Dressel 20 amphorae and marketed to most parts of the western empire during the first three centuries AD. Indeed one of the

most significant features of this volume like its predecessor, is that the large numbers of stamped amphora handles found on different sites have allowed the individual kiln sites to be recognised.

Ponsich has listed some 620 sites on a scale of 1:5, reflecting their declining order of importance – as reflected in surface pottery scatters. The description of each site consists of a summary of the finewares and other important finds encountered, together with an attempt to outline its chronology. Against the background of previous Spanish attempts at co-ordinated field-survey (very rare, and usually on a very small scale; the conception of site recording is best exemplified in the tortuously slow *Carta Arqueologica de Espana* series), this work must be held up as an example to follow. However, one does feel that Ponsich's definitions for the grading of sites is too fine (reflecting the unsuitability of uncontrolled surface collections for this kind of analysis), and that on the basis of the published evidence, 'Les fermes', 'Les abris' and 'Les sites incertaines' could have been grouped into one category.

Given the quantity of data in this volume and its predecessor, it is a shame that the author neglects to draw any conclusions from his material. In this respect we have to look elsewhere.¹ Several points do emerge, especially the great impact of Roman settlement in the area in the early Empire. With imported Italian *Black Glaze* pottery ('Camapanian ware'), providing the clearest evidence of occupation in the 2nd and 1st centuries BC, it is surprising to find so low a proportion of sites beginning in this period. What then was the economic significance of towns like *Italica* and *Corduba* in the 2nd and 1st centuries BC? It is also interesting to note that the total number of sites with late material is quite low, and that in a province well known for its wealth and importance in the late Roman and Visigothic periods (5th, 6th and early 7th centuries AD), the latest occupied sites are predominantly of the 4th and 5th century AD (*African Red Slip* ware, Hayes form nos. 61A, 61B, 59B, 73, 67 and the 91 series). This contrasts strongly with material from rural sites along the east coast of Spain – for instance in the *Conventus Tarraconensi* (modern Catalonia), which indicates occupation of the 5th to later 6th centuries AD.

This work is an invaluable contribution to the archaeology of the western Mediterranean, not only as a catalogue of amphora stamps from kiln sites along the Guadalquivir, but also as a record of sites that are rapidly being destroyed by modern agriculture.

¹ For instance, see Ponische M. *Nouvelles perspectives sur l'Olivier du Bass Gaudalquivir dans l'antiquité. In Production y Comercio del aceite en la Antigüedad*. Madrid, 1980, p. 47.

SIMON J. KEAY

ROUANET-LIESENFELT, A. -M. *La civilisation des Riedones* (Supplement à 'Archéologie en Bretagne' 2). Brest, 'Archéologie en Bretagne', 1980. 297 pp., 47 figs. Fr.80.00.

This book is very French and very traditional. The contents and the sequence of presentation in themselves make its character clear: manuscript sources, epigraphic sources, bibliography of the Riedones, the Roman conquest, the period of independence, the frontiers of the *civitas*, geography (principally the positioning of ancient place-names), Roman roads in the area, Rennes, Gallo-Roman rural settlement in the area (very little survives), modern names as evidence for Roman establishments, evidence of centuriation in the modern roads, coins and stray finds, discussion of the local government and citizenship of the Riedones, trading relations, coin hoards, religious cults, the end of Roman occupation. Obviously it is comprehensive in its way, but the emphasis is squarely in the traditional fields of epigraphic and literary historiography. Evidence on the ground is subordinated to the questions of ancient place-name location and such typically French pursuits as the consideration of etymological and ecclesiastical evidence for the limits of the *civitas*, of the squaring off of maps into centuriated land (cadastration). Compared with the British equivalent, e.g. Cunliffe's *Regni* or McWhirr's *Roman Gloucestershire*, the approach is only too evidently non-archaeological, which is sad considering the wealth of questions begging to be answered through archaeological means – what was the relative material wealth of the Riedones, how does the rural settlement relate to roads and urban centres, what was the economic basis of the area, was the *civitas* a coherent geographical unit definable by archaeological distributions, etc.?

All is not lost, however. There is a most useful collation of the evidence for the religious cults in the *civitas*, which in themselves are probably the most interesting aspect of the Roman

archaeology of this area. The inscriptions cast a valuable light on the organisation of a tribal cult, in this case dedicated to Mars Mullo and the *domus divina*. Their significance is admirably drawn out by André Chastagnol in an appendix and by Mme Rouanet-Liesenfelt herself in the section on Rennes. Other appendices also add significantly to the main text, especially those by Langouët and Galliou on archaeological discoveries made since 1960 when the original work was done.

On the whole, the book is a very useful compendium on an area of France that is becoming increasingly well known through the activities of the publisher 'Archéologie en Bretagne'. With the reservation that the approach may not be particularly up-to-date, *La civilisation des Riedones* is well worth reading by students of Roman Britain for information on part of a region of the Empire that was probably more like Britain than any other.

ANTHONY KING

SCORPAN, C. *Limes Scythiae: topographical and stratigraphical research on the late Roman fortifications on the lower Danube* (BAR Int. Ser. 88). Oxford, British Archaeological Reports, 1980. 219 pp., 25 figs., 57 pls. £9.00.

Scorpan discusses the limes forts in the Romanian Dobrogea, a geographical restriction which, although it represents only the eastern part of Moesia Inferior, is not too serious a failing since most of the archaeological evidence relates to the 4th–6th centuries when the region does roughly correspond to the Diocletianic province of 'Scythia Minor'. Chapters 1 and 2 briefly summarise the archaeological evidence for the Danubian forts and fortifications of the interior with particular 'comparative notes' on a range of sites from Moesia II and Dacia Ripensis, a collection which has no coherence and no obvious connection with Scorpan's theme. Particularly unfortunate is the omission of Abritus, which is certainly relevant to the Dobrogean system. Nor are the summaries accurate. For Iatrus, Scorpan argues that occupation of the fort continued throughout the 5th century (p. 82). The evidence of excavation has, however, argued strongly in favour of a hiatus between periods C and D, i.e. middle to late 5th century (K. Wachtel, Iatrus-Krivina (1979) p. 18). Chapter 3 summarises his conclusions. Typological analysis; tower shapes are described as 'Diocletianic' or 'Constantinian', a deduction for which there is insufficient evidence (p. 107). Plans of forts are included, most of which lack scales (p. 149, 150, 156, 158, etc.). The 19th century plan of Troemis is rightly dismissed as fanciful (p. 31) although Scorpan produces two plans of the eastern citadel, based on the aerial photographs, which purports to distinguish between 4th and 6th century schemes – an even graver delusion! His use of epigraphy is equally at fault. The fact that a veteran's tombstone has been found at Sacidava and legionary tiles of XL Claudia, can hardly be used to prove 'influence' by that legion over the auxiliary fort.

Most illustrative of the deficiencies of this book is his discussion of his own excavation at Sacidava. Scorpan states his purpose (p. 2) as providing 'a stratigraphically based chronology' for Sacidava and a re-interpretation of other fort excavations. The east gate has been simplified in his plan (plate 19, p. 164) and anomalies omitted (cf. Pontica 10 (1977), 229–51). Scorpan is not surprisingly vague about the stratigraphy of the east gate (p. 52) since it is clearly more complex than he is prepared to admit. Interpretation is also at fault. The late addition of short walls behind the towers is described as strengthening when it should be interpreted as the addition of permanent staircases to allow entry directly into the towers. His confident assertion that Sacidava has produced the best evidence for continuous 5th century occupation (p. 73) has not been established by excavation and his published 'stratigraphical sequence' supports no such conclusion (cf. Pontica 5 (1972), 301–27). The interesting valli seen on an aerial photograph of the site (plate 20, p. 165) are not discussed and he fails to discuss why three of them seem to relate to a defended area east of the fort and not to the fort itself.

These examples illustrate the uneven nature of composition and failures in interpretation both of his own and other excavations. The use of 'stratigraphy' is far from being the new insight into dating limes forts as Scorpan himself suggests. He has recently been rightly criticised for his work at Sacidava (P. Diaconu, *SCIV*, 31(i) (1980), 123–30) and it must be admitted that Scorpan has produced a work which is at best misleading and does no credit to the subject nor the quality of excavation particularly at Dinogetia and Tropaeum Traiani which have yielded important results which

BOOK REVIEWS

have been given far better treatment in the Romanian reports than his summaries in English would suggest.

A. G. POULTER

ARISCESCU, A. *The army in Roman Dobrudja* (BAR Int. Ser. 86). Translated from the Romanian by Nubar Hampartumian. Oxford, British Archaeological Reports, 1980. 225 pp., 5 maps. £10.00.

This volume, first published in Romanian in 1977, discusses the evidence for legionary and auxiliary forces in this part of Moesia Inferior during the 1st–3rd centuries AD (chapters 1 and 2). The evidence, for the 4th–6th centuries, inevitably more limited in quantity and dubious in quality, is reviewed and supplemented by brief discussions of roads, an attempt to locate sites, and notes on religion and Romanisation (chapter 3). Of most use are the prospographical and epigraphic appendices since much of the published material is not readily available outside of Romania.

Irritating for the 2nd–3rd centuries AD is the very treatment of the Dobrogea as if it were separate from the rest of the province, a deficiency particularly annoying since many of the units cannot be firmly assigned to the Dobrogea as they could equally have been stationed in the North Bulgarian Plain.

The epigraphic evidence is often used unwisely. A veteran's funerary inscription from Tomis (Constanta) does not prove that his former unit, Ala I Asturum, was anywhere close to the city (pp. 20, 36). The revival of the assumption that the distribution of legionary tile-stamps illustrates the 'influence' exerted by the legions has no proven foundation (pp. 11, 14). The production of legionary tiles and their supply to auxiliary garrisons, with a preference for downstream locations, need reflect no more than the availability of tiles during a particular construction programme. For example, the production centre for legionary tiles of XI Claudia was close to the legionary base of Durostorum near Oltna. Hence, the appearance of tiles of XI Claudia and V Macedonica at the same sites (e.g. Capidava) raises no administrative problems, but simply suggests two consignments of building materials from both legions. Similarly, the discovery of tile-stamps of all three Moesian legions at Callatis (Mangalia) is no evidence for legionary garrisons or 'influence' (p. 15).

Tile-stamps of auxiliary units are used injudiciously. It is true that one, possibly two, tile stamps of Ala I Pannoniorum have been found at Troesmis. There is no reason to believe that the unit must have been there before the legion arrived, probably after the 2nd Dacian War (AD 105/106). It could as easily have accompanied the legion to its new base. Tile-stamps of the Classis Flavia Moesica, V Macedonica, I Italica and cohorts II Mattiacorum have been found at Barboşi, at the mouth of the Siret. Aricescu argues that these finds prove the use of this base by these units (pp. 31, 11, 15, 28). An incautious deduction. All these tiles also occur across the Danube at Dinogetia. The re-use of tiles or the shipping of auxiliary and legionary tiles from one site to the other or even from further afield in the case of the legionary examples is the more likely explanation and perfectly illustrates the problem of using tile-stamps to identify units' bases, especially for sites along the Danube where transport of tile is relatively easy.

The deficiencies of the book in the interpretation of the epigraphic evidence is most evident. The attempt to locate auxiliary troops in the Dobrogea as early as the Flavian period is as yet unsubstantiated by any archaeological evidence (with the possible exception of the naval base of Noviodunum) and epigraphic finds from the Black Sea coast do not provide conclusive proof when the evidence from the Danubian limes itself is, so far, largely negative. However, as a corpus of material, Aricescu has produced a book which will surely be of help to the student of Roman military history on the Lower Danube, even if his deductions need to be treated with the greatest caution.

A. G. POULTER

CARSON, R. A. G. *Principal coins of the Romans*. Vol. 3. *The Dominate, AD 294–498*. London, British Museum Publications, 1981. 112 pp., illus. £20.00.

With this third volume the series of *Principal coins of the Romans* is complete. It is now possible to follow in words and pictures the coinage of the Romans from the 3rd century BC to the

reform of Anastasius in AD 498; and although there are one or two works with the same scope, the simplicity of these volumes and the highly compressed nature of their presentation makes them an ideal starting point for the student.

This theme of completeness must be the major reason for approval for, until the last few years, the student has had no clear guide through the coinage of the 5th century. Now, with this work, and Kent's *Roman coins*, the development of the coinage can be seen as a single whole in which the several metals, periods, mints, emperors and standards fit together in an evident continuity. Such a scheme of presentation must be perfect to secure complete approval: no humanly produced book is perfect, hence a list of criticisms. But the criticisms are details to set against the substance of a very real achievement.

The standard of the illustrations, in which each coin is shown at its actual size with a minimum of commentary, has been a constant point of complaint, in which this volume is no exception. In general, the larger, and the later the coins, the better the photographs. Coins 1155, 1160 and 1171 seem to have illumination from only one side so that half the coin is shiny highlight while the rest is murky shadow. Coin 1253 is an almost perfect example of this problem taken to the limit of a very bad photograph. And yet the later gold and silver of the 4th and 5th centuries is clear, crisp, and detailed. It may have something to do with the higher relief of the earlier coins, or it may just be a patch of bad technique.

The other bone of contention throughout the series is the price. With only 104 pages including illustrations a price of £20 compares very unfavourably with several other series of books which include substantial illustration, such as the Britannia monograph series, and the blame for this must be set on a very strange pricing policy of British Museum Publications. If only they would aim to sell many copies at a moderate price, rather than a few copies at a high price, the world of scholarship would be much better served.

Points of detailed criticism in a work as wide and full as this may tend to personal preferences, but I shall try to keep this in check. Every reviewer will find points of interest and coins which he considers culpably omitted, but, to get the coinage into the space available choices had to be made, and one person had to make those choices.

One major surprise in this volume is the lack of coins of the mint of Alexandria: only two are illustrated between 313 and 395. This immediately highlights a point which is not at once obvious from the title. Although the book contains the principal coins of the Romans it is limited to those which are in the British Museum, and even that great collection has its strengths and its weaknesses.

The letters and signs on the coins are always transcribed and printed, but a rather erratic selection is explained. Thus attention is drawn to the mark KV as perhaps showing a counting mixture of K (= 20, Greek) and V (= 5, Latin) on 1215, but the more usual mark XXI on 1194 is not discussed, and the Greek equivalent, KA, is not apparently shown. The same sort of problem arises later on when the mark OB (obryzum or refined gold) is only explained on 1427, the fifth coin on which it appears, and the companion letters COM (for Comitatusian mint) do not seem to be explained at all. I missed any comment on the variations in silver standard around the year 313, which Kent would raise to the level of the introduction of a new denomination, the complete omission of Theodora seems odd when Helena and Fausta are fully included, and I would like the removal of silver from the bronze coinage around 356 to be made explicit, rather than doubtfully implicit.

There are some misprints, but few of them will cause any real hardship to discerning readers. Thus I suspect that 1210 has the mint-mark KA rather than delta which is not recorded in the relevant RIC number, 1256 is dated 305–6 rather than 315–16, Constans is killed in 350 rather than 360 (p. 40), and 1342 presumably belongs to 342–3 as in the heading rather than 346–7 as below.

Of more substance, but also more subjectivity, is my complaint that no use has been made of the placing of the illustrations so as to teach. The common issues of 330–5 are split up between different mints and their connection is lost; the three substantive issues of 364–78 do not occur together, and only the student who already knows the coinage will connect them up as they were connected in Roman production, Roman use, and Roman loss.

And yet the series is complete: it will be invaluable to students who, having learned from it, still have the opportunity to write the perfect guide to Roman coinage.

RICHARD REECE

BOOK REVIEWS

CARSON, R. G. and KRAAY, C. M. (eds). *Scripta nummaria romana: essays presented to Humphrey Sutherland*. London, Spink and Son, 1978. xiii + 250 pp., figs., 24 pls. £20.00.

This volume brings together 15 papers written to honour one of the pioneers of modern numismatic studies. Virtually all on Roman subjects, they range from the practical to the esoteric, and together form a readable group of studies from which the Roman numismatist can gain a greater or lesser amount of profit. Three articles in particular are of importance. The first, Daphne Nash's 'Currency in central Gaul from Julius Caesar to Nero', is a constructive examination of the coinage in a non-militarised zone of Gaul, well illustrated with plates and distribution maps, and discussing the coin distribution of the Central Gaulish tribes, the interrelation of their coinage with the official Roman issues and the imitations of the Claudian period, and the probably autonomous control of local fiscal affairs by the tribes during this period. The second, 'Bronze coinage in Roman Britain and the Western Provinces', is another example of the extensive groundwork being produced by Richard Reece, before whom Romano-British studies seemed to have ground to a halt; the voluminous statistical work of which this article is a part is the base from which all recent work has started. Here, Reece looks into the supply of coinage to the Western Provinces from the official mints during the period c. AD 330–400, and examines why coins from the various mints might occur in Britain, and other provinces, in the proportions they do; clearly illustrated by graphs and tables, it once more demonstrates the importance of the 'back-to-basics' approach which must be adopted if we are to attempt to discover the information hidden in archaeological coin finds and hoards. The third article, Ian Stewart's 'Anglo-Saxon gold coinage', is an impressive and detailed appraisal of present knowledge of the coinage of the 7th and 8th centuries, although for anyone unfamiliar with the coinage under discussion the lack of any illustrations was a drawback. By contrast, the other descriptive articles in the volume benefited from good illustrations, particularly David MacDowell's 'The Julio-Claudian mints at Rome', Philip Hill's paper on the monuments of Rome illustrated on coins of the Severans, and Colin Kraays's 'Bronze coinage of Vespasian'. Mention should also be made of Cathy King's useful and painstaking study of the metrology and role of denarii and quinarii in the later 3rd century, and of R. A. G. Carson's critical study of mid-3rd century antoniniani in which he reattributes to Rome a high proportion of the coins formerly assigned to other mints, on the basis of die-link analysis. Articles by Michael Crawford on Adriatic trade in the Hellenistic period, John Kent on Urbs Roma medallions, Patrick Bruun on the relative values of hoards and site finds as indicators of currency in circulation, Anne Robertson on the circulation of Roman coinage in northern Britain, Barbara Levick on the use of the 'Concordia' slogan on coinage, J.-B. Giarl on the use of Roman imagery on French Renaissance medallions and Cornelius Vermeule on the use of the imperial shield in Roman numismatic art, complete the volume. As the book has now been remaindered at a price to please even the meanest, it should now, deservedly, be within the reach of many students doubtless originally deterred by the £20.00 price-tag.

MICHAEL HAMMERSON

LINDGREN, C. *Classical art forms and Celtic mutations: figural art in Roman Britain*. Park Ridge, NJ, Noyes Press, 1980. xi + 148 pp., 96 pls., 14 figs. \$24.00.

This book is a revised version of the author's dissertation for a PhD degree at Columbia University, New York City. As such it won the 1978 Prix de la Confédération Internationale des Négociants en Oeuvres d'art and as such it still reads like a university thesis. The book is divided into three parts. Part I, dealing with the setting, that is, pre- and post-conquest Britain, is seemingly written as an introduction for American readers and contains little that is new. There are two maps, one showing the 'major civitates and tribes' which surprisingly includes Housesteads, Isca (Caerleon), Fishbourne and 'Lindinis', and the other revealing the highly selective distribution of the objects chosen; only seven are from the Wall region. Part III contains the analyses and conclusions with two rather complicated flow charts; in chart II, entitled 'A Collation of Catalogued Representations and Environments', a somewhat heavy prominence is given to the Druids. The main conclusion is that 'although the Romano-British style' (here defined as an 'amalgamation of Celtic and Mediterranean traits at any level of analysis') 'existed in the I and II centuries it is in the III and early IV centuries that it becomes

the dominant form in figural representation eclipsing the Mediterranean style but not completely replacing it'. This may be the case for certain of the objects selected for this book but there still remains two problems, that of the precise dating of individual pieces, particularly in the case of the bronze figurines which have often to be dated on stylistic grounds alone, and that of the precise consideration or definition of the Romano-British artistic style. The term 'Celtic mutations' used in the title referring to the modification or transformation of the art forms of the classical world, may, indeed, seem a somewhat disparaging one to archaeologists of Roman Britain. Although some reference is made in the prologue to some of the Celtic cults, such as that of the Head, their significance is not fully appreciated so that the relief from Littlechester (pl. 25) is discussed entirely as a crude rendering of the classical figure of Mercury and not in a possible context of the horned god. Such a method illustrates the danger of confining discussion of art forms in Roman Britain to stylistic criteria.

Part II contains the substance of the book. It is a catalogue of selected reliefs and figures relating to Mercury, Mars, Venus and Minerva, and here the book does perform a useful function. Each object is accompanied by a photograph and a description which includes dating along artistic lines. The selection of these objects has been confined to those in major museums and, in particular, to the bronze figurines, with only a very small proportion of relief sculpture. This selectivity may have distorted the emphasis; it certainly means that occasionally an important fact is omitted. The heads of Minerva found at London (pl. 68) and at Silchester (pl. 72) may have performed the function of sceptre heads but they need to be compared to that found at Ospring in a funerary context similar to the one at Remagen (*Bonner Jahrbücher*, 116(1907), 117). It also seems a little strange to include a bronze female head (pl. 169) in the section devoted to Minerva and then to say that it has been mislabelled. The head is an apparently reduced version of an ornamental part of a warship, probably the name-embell depicting Victoria (cf. *Fogg Art Museum Newsletter*, 1966). The identification of the Mercury from Benwell (pl. 31) might also be disputed. The head is paralleled by one from Caistor-by-Yarmouth, now in the Castle Museum, Norwich, and the so-called wings on the head might equally be interpreted as ivy leaves and berries as found on busts of Bacchus (cf. *Gallia*, 10(1952), p. 31). It would however, be churlish to continue to discuss misrepresentations as there is clearly room for interpretation. This book, with its excellent photographs, does provide the basic material which will allow further consideration of the evidence and stimulate discussion of its evaluation.

JOAN P. ALCOCK

KILBRIDE-JONES, H. E. *Celtic craftsmanship in bronze*. London, Croom Helm, 1980. 266 pp., 83 figs. £15.95.

This is a very well-presented book tracing the development of Celtic art in Britain from c. AD 1 to the end of the 7th century. Over this large time-span Mr Kilbride-Jones has made an attempt to grapple with the Romanising effect on Celtic art, its survival on the fringes of Britain, particularly in Ireland, during the Roman occupation, and the re-emergence of the art on hanging bowls and other bronzes at the end of the Roman period. The text is lavishly supplemented by the author's good clear drawings of the objects discussed.

Kilbride-Jones first introduces the motifs in Celtic art, such as lotus-bud, tendril and palmette, and then discusses patterns such as spirals and chevrons. The rest of the book is divided roughly into periods: Celtic art styles and objects prior to the Roman invasion; those used after the Conquest, until AD 196; and post-Occupation styles. The section on pre- and early Roman styles begins with a description of three of those styles, which he calls Galloway, Elmswell-Brighter and the Aesica style (based on the Aesica brooch). These styles are not really brought into the discussion in the remainder of the book and would perhaps have been better placed in the introduction.

The bulk of the book consists of chapters on the various groups of decorated objects: most useful are the individual chapters about dress fasteners, dragonesque brooches, penannular brooches and, of course, hanging bowls, although much reiterates already published work; and also the summaries of the evidence from Ireland. Occasionally extra references would have been useful, for example, many of the objects from northern Britain are discussed by MacGregor (1976) and one would expect the section on torret decoration to include a reference to Leeds (1933). The author

BOOK REVIEWS

illustrates many of the objects discussed, though one is tempted to ask whether some were taken from published drawings: the dragoesque brooch from Rudston (fig. 55:10) is a flat brooch not a length of bronze wire (Stead, 1980, fig. 61, no. 16).

The period in the development of Celtic art covered by this book is one that has been neglected and a survey of this kind helps to fill the niche. Unfortunately, there are certain aspects of the book with which one is not entirely happy. The first problem concerns the style, and in particular the author's rather subjective approach to his material. He considers the Roman period to be one of degradation and decay, and is dogmatic about the destruction, by the Romans, of the Celtic art style, describing Classical art as a degraded commercial variety (p. 258). Occasionally his phrasing is anachronistic, and some of his dogmatic statements, especially in the early part of the book, are difficult to accept; for example (p. 71), that the Roman merchants penetrating Britain prior to the Claudian conquest were *quite clearly spies* (my italics). His opinion of the Romans colours the entire discussion, and the result is a rather biased account.

Another problem involves the references. Many of the references, in the form of notes at the end of each chapter, are given in an inadequate form: often with no date or author's initials. The Harvard system with a list of references at the end of the book would have been preferable. Many of the references are now out of date and the conclusions that follow from them cannot now be accepted, very few physical anthropologists would today believe that the dolicho-cephalic skulls of the Glastonbury people indicate that they were of Mediterranean race (p. 114). Further, there is no evidence that Belgic settlers in Hertfordshire (if they existed at all) developed farmland ignored by the Iron Age peoples of Britain (p. 258), or that they introduced enamelling on metalwork (note 1, p. 107).

One further criticism concerns the author's Chapter 2 on factories (pp. 34–8); in it Kilbride-Jones asserts that three factories existed for the production of metalwork in Roman Britain: at Nornour, Scilly Isles; Traprain Law, mid-Lothian; and Settle in Yorkshire. The evidence for a brooch factory at Nornour as put forward by Hull has been discounted by Butcher (1977) in a recent discussion of Roman enamelling, following modern excavations there. The other postulated factories at Traprain Law and Settle are not convincing from the arguments given; Traprain Law yielded perhaps 30 moulds and some evidence for enamelling, but not enough to prove the existence of a factory (Burley, 1955–6). Kilbride-Jones' arguments based on distribution maps are also unconvincing. For example, he argues that certain dress fasteners (fig. 44) were made in Brigantia, though without any strong production evidence. The examples of such dress fasteners in southern England are explained as traded items, and the merchants are described as trickling by subterfuge down the Icknield Way (p. 168). 'Either the Romans turned a blind eye to its continued use, or else they were unaware of its existence'. Surely this is a very simplistic view of the market mechanisms operating in Britain during the Roman period?

Mr Kilbride-Jones has presented the reader with a personal review of late Celtic art, of which the major criticism is, unfortunately, that he fails to take full account of recent approaches. Recent work on the technology of the early craftsmen and new methods of analysis of Celtic art (eg. Duval and Hawkes, 1976) have superseded the approaches pioneered by Jacobsthal. It is useful to have a summary of the late Celtic art of England and its connections with the early Celtic art of Scotland and Ireland, and it is a pity that the style of the book overshadows its usefulness.

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JENNIFER FOSTER

BOOK REVIEWS

HIGGINS, Reynold. *Greek and Roman jewellery*. 2nd edn. London, Methuen, 1980. xlv + 243 pp., 28 figs., 64 pls. £18.00.

This is the second edition of a handbook first published in 1961, and it retains both the virtues and the faults of the original publication. On the positive side an enormous number of facts are packed into relatively few pages. The bibliographies and site lists are well chosen and have been brought up to date. (In the Roman section, however, the treasures of Lyons, Backworth, Rhayader, Sully Moors and New Grange are still incorrectly described as tomb groups which they are not). The chapter on techniques is masterly; here Dr Higgins evidently feels most at home, and anyone interested in ancient crafts should read it. The photos are an improvement on those in the first edition; it is good to have more objects not in the British Museum collections included, though a pity that the plates were assembled just too early to include items of 4th century BC royal jewellery from Vergina.

Criticism starts from the fact that much of the text is not very readable having been written virtually in note form. The companion volume by the late Professor Donald Strong on Greek and Roman plate is penned in a far more relaxed style and yet the problems of writing a handbook on silverware are not very different from those encountered by the jewellery historian. Secondly the title is rather misleading, for Dr Higgins does not deal with jewellery in base metal especially iron and bronze or in jewellery of non-metallic substances such as amber, jet, glass and enamel except in passing. Nor is the outstanding art of the gemcutter dealt with in this survey. It really is special pleading to leave them out, for all these materials were employed in the beautification of women (and sometimes of men) and a bronze bracelet can flash like gold, an iron ring can look like silver, and *jewels* are after all the essence of jewellery. In any case while gold jewellery is seldom found by archaeologists, bronze jewellery is found on even the grubbiest Romano-British site. Finally, the treatment of the Roman period is far too cursory. Twelve pages scarcely justify the inclusion of the word 'Roman' in the title, and a separate handbook is badly needed here.

It is a pity to end on so unenthusiastic a note, but candour forces the reviewer to say that when attempting to learn about some new item of jewellery, he turns first to F. H. Marshall's remarkable catalogue of jewellery in the British Museum (1911, reprinted 1969) and then to A. Greifenhagen's splendid catalogue of jewellery in West Berlin (1975). Higgins' handbook tends to remain unopened on the shelf. Others may find it more useful, for it is after all the only handbook of its type available.

MARTIN HENIG

SHELTON, Kathleen, J. *The Esquiline treasure*. London, published for the Trustees of the British Museum by British Museum Publications, 1981. 104 pp., 29 figs., 48 pls. £40.00.

It is extremely difficult to ensure a fair review for Dr Shelton's work because there are so many things that count against her, and many of those are not her fault. Thus the book is announced as a study of the late Roman silver treasure, mainly in the British Museum, at a price of £40. It is reasonable to expect a fairly bulky tome, with a very full catalogue, some detailed illustration and analysis of decoration, both figural and pattern, some thoughts on the place of the treasure in late Roman art, and perhaps some scientific appendices on the metals used, and the methods of manufacture.

What in fact we get are 48 plates of excellently clear photographs, but only nine of these could possibly be called details, two pages of index, four of bibliography, 26 pages of catalogue, and 70 pages of description and discussion which start on p. 11 and contain 9½ blank pages after that.

Consider the casual reader who picks up the book, looks at the pictures, and is attracted by the monogram on the circular plate no. 5 on plate 26. He wants to know the reading of the monogram so he looks in the index to see where it is dealt with. Unfortunately monogram is not in the index, neither is plate, circular plate, inscription or wreath. Undeterred by this cavalier treatment he finds the entry in the catalogue for no. 5, the circular plate, and sees to his joy a drawing of the monogram, and also another punched inscription which says that the plate is part of a set of four which together weigh five Roman pounds. Unfortunately the catalogue entry does not expand or transcribe the monogram, nor refer to any section in the book which deals with it. This is, reasonably enough, to be found in a chapter headed 'The inscriptions within the treasure', Meanwhile the reader's attention has been

BOOK REVIEWS

caught by this weight. How accurate is this value of four Roman pounds? Four dishes, total weight five libra, about 400 g each. Unfortunately there are no mentions of weight in the whole book.

There are points at which the author gets down to very useful details, but she does this almost apologetically, as if such mundane points are not really the stuff of the study of the past. She finds an interesting recurrence of an eight-sided faceted and fluted wire on the hanging ring of the Proiecta casket, the rim of the circular plate, and the handle of the Pelegrina ewer. But we cannot check her ideas. There are no detailed drawings, the plates are all general shots, and it must be said that going only from the published photograph, the handle looks neither octagonal nor fluted. But this does not really matter. I am willing to take Dr Shelton's word, or, in desperation I can call into the British Museum and check for myself. Other readers will not be so trustful, nor are they so fortunately placed. The failure to document this fascinating little quirk of construction adequately means that other researchers will not be able to link their own discoveries to the workshop which Dr Shelton thinks produced much of this Esquiline treasure, without coming to observe for themselves. The same applies to the excellently detailed comparison, in words, between the central punch cut diaper pattern on Esquiline dish no. 4, and the similar pattern on the fluted dish from Traprain Law. The similarities are great, but for future reference we need expanded drawings of the punches and their cuts so that they may be firmly identified if they turn up elsewhere.

On more general themes Dr Shelton is much happier. She deals well with the rash of Renaissance which bedevil the writing on 4th century art, and shows that if they are all to be believed every deposit of late Roman silver demands a Renaissance of classical taste to account for it. She avoids the conclusion which I would draw which is that silver ware was decorated in the 4th century in a much more classical style than was applied to court sculpture, glasswork, or coinage. Her enquiry into an 'Esquiline' workshop is sane and reasonable, and her refusal to attach the treasure to epigraphically known people is highly to be commended.

If only she had been under the influence of a more earthy school of art history which had insisted on detailed chemical analyses of each piece (no chemical facts at all are quoted) and a thesis which grew out from objective, verifiable, and repeatable facts, the results, in her hands, could have been excellent.

RICHARD REECE

WALKER, Susan and BURNETT, Andrew. *The image of Augustus*. London, British Museum Publications, 1981. 48 pp., 46 illus. 1 map. £2.95.

As this publication consists of only 47 pages and has a paper cover I suppose it must be referred to as a booklet. And since it was produced to accompany the exhibition on the same theme mounted in the British Museum, it inevitably becomes the booklet of the exhibition. It is considerably more than this, and in fact contains more of use and substance than many art-historical productions of 10 times the length.

Susan Walker would presumably claim to speak on the sculpture and the monuments and Andrew Burnett on the coins and general portraiture, but the two authors have created a text which reads as a single unit. This traces the idea of kingly portraits from the Greek world, through Alexander into the Hellenistic kingdoms, and amalgamates this tradition with the very different, and much more sober tradition of the portraiture of worthy men in Republican Rome. The scene is set for the Imperatores, Caesar, Marc Antony and Pompey, and thence to their successor, Octavian. Octavian is shown transforming himself by a programme of portrayal into Augustus, and the variations of Augustan portraiture are examined briefly. Augustan monuments, and portraits of the Julio-Claudian family act as post-scriptive essays, and two end maps show us where portraits of Augustus have been found, and where coin portraits of Augustus were minted.

The touch throughout is light but firm. Ancestor portraits are demoted into a Roman Republican 'style', and the illustrations of the Ara Pacis contain no pointers to portraits of Augustus, but if I had been Andrew Burnett, and if I could have secured my co-author's agreement, I should have been very much more insistent on the unique value of coins as the only possible way of identifying any marble bust as a named person. The half tone plates are clear, and make their points with great effect. Authoritative, readable, and well worth buying.

RICHARD REECE

BOOK REVIEWS

GODWIN, J. *Mystery religions in the ancient world*. London, Thames and Hudson, 1981. 176 pp., 142 pls. £10.00.

Joscelyn Godwin introduces the book by declaring himself to have 'the highest respect for the industry and dedication of our archaeologists and classicists, but not for the attitude that approaches the Mysteries in the same spirit as the classification of potsherds . . . Is it possible to comprehend that in which one does not believe?' This reviewer applauds so forthright a statement, because one of the most compelling reasons for studying antiquity is surely to enable poor, spiritually impoverished modern man to recover the religious and philosophical insights of sages like Socrates, Plato and Plotinus, to think on the lofty and gentle syncretism of the cult of Isis and to recover the ecstatic revelation and optimism of the Dionysiacs. Symmachus was indeed justified in writing 'uno itinere non potest perveniri ad tam grande secretum' (*Relatio*, 3, 9). The author is again surely right in refusing to circumscribe the Mystery Cults too narrowly for in many respects the veneration of Mithras and Cybele differ little in kind from other cults of the Empire: it is only that they are 'oriental' and therefore appear exotic in the Roman West. Sections on the Roman Gods, Philosophy and the Imperial Cult are rightly included alongside Christianity but Kabbalistic Judaism, about which incidentally 'most Jews' are far more sympathetic than Joscelyn Godwin appears to believe is medieval.

Thus far there was the making of an adequate introduction to Roman Imperial religion but unfortunately real insight has been overlaid by the stale conventions and the astrological superstitions of the occult. 'Perennial Philosophy', the 'Age of Aries (second-first millennia B.C.)', the 'Age of Pisces (second-first millennia A.D.)' and an attack on modern scholars 'devotees by profession of Apollo' whose 'efforts to understand [Dionysus] are vain, for he is innately hostile to rational thought' should have no place in a serious archaeological study. When we read that 'Serapis was the presiding deity of Alexandrian civilization . . . just as Ammon was the god of Thebes, Athena of Athens, Hagia Sophia of Byzantium [*sic*], and *Wotan of the Third Reich*' (my italics), the implications are actually rather unsavoury as well.

However the photographs (unfortunately without scales or dimensions) are often of great interest even if the captions are not always accurate. For instance Aurelian's solar crown illustrated on a coin (plate 26) was by no means his own innovation and appears on *dupondii* and above all *antoniniani* of the earlier emperors. Also why is the terracotta *Dea Nutrix* (plate 77) from Gaul included in a chapter on Cybele?

Regretfully this volume cannot be recommended as an introduction to the Mystery Cults: the student is far better served by John Ferguson's *Religions of the Roman Empire*, R. E. Witt's beautiful and poetic study of Isis, Maarten Vermaseren's works on Mithras and Cybele and of course A. D. Nock's great classic *Conversion*.

MARTIN HENIG

HASSALL, M. W. C. and IRELAND, Robert I. (eds.) '*De rebus bellicis*'. Part I. *Aspects of 'De rebus bellicis': papers presented to Professor E. A. Thompson at a seminar held in London, December 14-15, 1973*. Part 2. '*De rebus bellicis*': the text (BAR Int. Ser. 63). Oxford, British Archaeological Reports, 1979. 131 and 196 pp., 23 pls. £9.00.

This BAR Supplementary volume arises from a seminar held at the London Institute of Archaeology in 1973. Its subject matter, the *De Rebus Bellicis*, was a product of the Later Roman Empire, as was the document with which it was transmitted to modern times in the *Codex Spirensis*, the *Notitia Dignitatum*. Hence the new volume constitutes a natural companion to the earlier BARS on the *Notitia*.

The volume contains two parts, the first being a collection of papers (edited by Mark Hassall) on various aspects of this extraordinary pamphlet, the second being a text and new translation by Dr Robert Ireland, with voluminous commentaries, indices and analyses.

The conclusions of the various papers have been drawn into the beginnings of a new, coherent picture of the Anonymus and his message by the late C. E. Stevens in his summing-up. It becomes clear that the descriptions and illustrations of extraordinary war machines for which the Anonymus is best known (and here the subject of an entertaining study by Mark Hassall), were in fact merely window dressing to disguise the real message of the pamphleteer, who was clearly not a soldier. But

he was attempting to address one, a soldier Emperor (probably the Emperor Valens if we accept Alan Cameron's convincing arguments), so he cunningly sugared the pill with military curiosities to engage his intended readers' attention. By this means, he hoped to reach the Emperor's ear with his actual appeal for economic reform. His ideas on this topic are the subject of papers by Patsy Vanags and Richard Reece. Space precludes detailed discussion of these and the several other papers presented, so suffice it to say that the papers in part one mark a major advance in our understanding of the document, and are well worth reading to gain an insight into the attitudes of a 4th century provincial (probably a decurion) towards defence, government and economics.

Part two of the volume is entirely the work of Dr Robert Ireland, in which he presents the text and a completely new translation and then proceeds to dissect the former in the most meticulous detail. He includes a section on the transmission of the text, which is itself a fascinating piece of detective work with implications for our understanding of the *Notitia* as well as the Anonymus. By intricate argument Ireland seeks to establish that Ghelens first printed edition of the *Notitia* was taken directly from the *Codex Spirensis*, which he suggests may have become lost because Ghelen broke it up to use as printer's copy. There is a catalogue of printed editions of the *De Rebus Bellicis* and a very extensive textual criticism, accompanied by analyses of composition, grammar, vocabulary and much else which will be of limited interest to the non-specialist. This work of analysis is so immense and detailed that one suspects that BAR was the only format in which it could ever have been published. However, as Dr Ireland himself observes, 'it was necessary to do all this now, because none of it had been done before' (part two, p. iv). My only criticism, and it is a minor one, is that text and translation were not printed on opposing pages, but separately, which does make them cumbersome to use. I was also rather surprised by a conclusion of some significance which Dr Ireland draws. He believes that the Anonymus 'was an army man, perhaps an officer, perhaps connected with the artillery . . .', despite the words of the Anonymus himself, in Ireland's own translation; 'Those who know war through personal experience will tell better than I what destruction devices of this kind [i.e. the war machines] can wreak among the enemy' (*De Rebus Bellicis*, XII, 4).

Despite these quibbles, it is a remarkable piece of work and the definitive statement on the Anonymus. The new volume will certainly become a mandatory reference work for students of the Later Roman Empire.

SIMON JAMES

DE JESUS, Prentiss S. *The development of prehistoric mining and metallurgy in Anatolia* (BAR Int. Sers. 74), 2 vols. Oxford, British Archaeological Reports, 1980. 495 pp., 29 figs., 23 pls., 6 tables, 11 graphs, 21 maps. £17.50.

This book is one of several written in the last 20 years or so by an archaeologist on metallurgy. The task is an ambitious one for it deals with two disciplines each demanding a high standard of academic ability and scientific knowledge of a specific kind, i.e. the knowledge and interpretation of ancient metallurgy, a closed book even for the modern metallurgist.

This is a logical sequel to S. Przeworski's *Die metallindustrie Anatoliens der seit, 1300–700 BC*, and summarises the metallurgical and archaeological evidence that has become available since the publication of the latter in 1939.

De Jesus was director of the American School of Anatolian Studies in Ankara and this work is based on his own fieldwork and shows the result of his close contact with the Turkish MTA (Turkish Mineral Research and Exploration Institute). It consists of two parts. The main discussion of the subject in Part I, and analysis and tables and geological maps in Part II.

The work opens with a discussion of the geological setting, followed by a description of ore types. The second chapter discusses ore types and the production of copper, the trace elements present in the copper ores of Turkey culled from unpublished reports in MTA archives. This contains a useful selection on prospection. It discusses the reactions that occurred during copper smelting taken from relevant textbooks and recent reports dealing with copper extraction. After this we come to the difficult question of smelting furnaces. No furnace from the prehistoric period has been found, although De Jesus has found and excavated one from the Roman or Byzantine period. Certain characteristics of this furnace suggested that it was a matting furnace, i.e. a furnace for the production of mixed copper and iron sulphides from which copper would be extracted at a later stage.

In view of the comparative absence of early furnaces, the author discusses those found at Timna, Israel, and the smelting experiments by Boydell and others in the UK. He goes on to discuss charcoal and other fuels and tuyeres and bellows. The chapter ends with a short section on moulds and casting.

Chapter 3 opens with a discussion on the nature and sources of tin ore. This subject has been under intense investigation by Wertine, Muhly and others. The problem is confounded by the claimed presence by Desch of 0.27% of tin in a piece of native copper from Ergani Maden. Recent analysis of native copper from the same source, and many others have failed to confirm the presence of tin. The presence of small amounts of tin (0.003–0.07%) (p. 56) is not surprising in view of the occurrence of small amounts of tin in many of the ferruginous gossans of the world, such as at Rio Tinto. But, if the gossan is used as a flux, tin is not likely to be present in the resultant metal in any great quantity.

The subject continues with a discussion of tin washing, ore dressing and of the occurrence of tin bronzes and tin objects such as the unique tin bracelet from Thermi and the later Mycenaean use of tin foil on pottery.

It is well known that silver occurs in the form of sulphides and chlorides, and mainly as a minor element in lead ores. The author discusses the widespread occurrence of lead ores and the early works of Gwlad. References are made to silver ore washing such as at Thorikos in Greece but no similar sites seem to have been found in Turkey.

Generally the evidence for early working of lead ores is poor like that of copper, although the author gives an early date for the cupellation of lead for the recovery of silver. At this stage we are treated to an extensive discussion of silver objects and their analysis.

The next section treats, astonishingly enough, gold and arsenic together. Perhaps it is useful to be reminded of the existence of arsenic as a native metalloid but the discussion of arsenical minerals is rather brief considering their possible importance in early copper metallurgy. And we are not given much data on Turkish deposits of this element which must surely exist in large amounts. Evidence of this lies in the slag from Gümüş containing 30% As. This is followed by the usual discussion on the poisonous nature and volatility of this element. Considering the wide use made of arsenical copper in the Bronze Age as shown by Eaton and McKerrell, this section is most disappointing. To return to gold, we are given a useful summary of the overall position in the classical world including that of Troy and the West.

The next chapter is on mining techniques in antiquity and might logically have been put earlier in the work. It has little on mining and much on metal mining tools.

Chapter 7 deals with the composition of copper-base metalwork in Anatolia. This is based on finds on individual sites.

The conclusion tries to summarise the previous chapter in logical sequence. Starting with native copper in Cayönütepesi in 7000 BC the author finds no reason to doubt that the 'slags' reported by the Austrian investigator from Çatal Hüyük are smelting slags. As he has made no attempt to define slags and no analysis is given we are not given in this work the evidence on which he has based his conclusion. But comparison of the analysis of the Çatal Hüyük 'slags' will show that it bears no resemblance to the normal type of copper smelting slag.

Bronze appears at Mersin in c. 4300 BC and the author supposes that the tin was obtained from local deposits. It is strange that whereas obsidian can be traded over considerable distances at about this period the author does not consider the same possibility for tin.

Starting with a malachite bead from Shanidar, dated c. 10,000 BC we reach the date of the use of native copper by 6–7000 BC at Tell Ramad I in Syria and Ali Kosh and many other sites. At much the same time we see the introduction of lead at Yarim Tepe.

The author seems to play down the importance of copper deposits in north and north-west Anatolia and claims that they could not have supplied the needs of Troy, etc., yet the deposits at Kastamonu had clearly been used as evidenced by slag heaps in the vicinity.

The author ventures the statement that smelting rarely took place in settlements. While this seems a rational statement in view of the fuel needs which he correctly estimates, it does seem to be contradicted by the evidence for copper smelting slag in town sites in Cyprus such as Enkomi and Kition. We have yet to find the sites associated with the large slag heaps away from the well-known settlements.

Unfortunately the author seems unable to distinguish between smelting and melting. While

somewhat dubious about the attribution by Caldwell of the site at Tal I Iblis, Iran, as a crucible smelting site he regards Abu Matar, Israel, as a smelting site and appears to be unable to recognise crucible slag where he sees it. He makes the mistake of regarding jarosite as a flux in lead smelting rather than the source of the silver in the silver-lead extraction process.

Generally, this is a useful assembly of recent data by an archaeologist who has worked in the area. His lack of metallurgical training shows in places, particularly in his confusion between melting and smelting slags and his lack of understanding of such things as speiss. It is unfortunate that he has not taken the trouble to acquaint himself with a modern work on extractive metallurgy where he could have found out why it is necessary to add silica to flux ferruginous gangues, and iron oxide to flux silicious gangues.

Although he is critical of others when surer of his ground, one could have wished for a more critical attitude to the conclusions of some of the excavators and scientists. The proof reading is occasionally slap-dash and the names of authors incorrectly spelled. As the author states, most of the book was written in France and the work clearly suffers from the author not being near his sources nor a centre where checks could have been made.

There is no doubt, however, that the book is a useful contribution to our knowledge of Anatolian metallurgy.

E. C. J. TYLECOTE

KENYON, K. M. *Excavations at Jericho*. Vol. III edited posthumously by T. A. Holland with contributions from R. Burleigh, I. W. Cornwall, G. Kurth and O. Röhrer-Ertl. London, British School of Archaeology in Jerusalem, 1981. 2 vols. Text, xxxiii + 540 pp., 17 figs., 9 pls.; Plates, 202 half-tone, 141 line-drawings, mostly plans and sections. £115 subscription price.

The volume is subtitled 'The Architecture and Stratigraphy of the Tell', and a footnote to the Introduction explains that 'the present volume deals mainly with the site descriptions. The finds, scientific reports not included here, conclusions, and discussions will appear in *Jericho IV*'. The time taken to prepare the vast amounts of material for publication, and the terrifying inflation affecting the price of books led to the decision to publish as soon as available the first part of what was intended to be the simultaneous publication of all the results of the excavation of the tell at Jericho. This practical compromise, however necessary, means that we are at present in the frustrating position of having the site itself at our disposal, for which we shall all be unutterably grateful, but having to wait for the finds and much more importantly the discussion and conclusions. For this volume is a mass of raw material which cannot be understood without the finds, and which can scarcely be read without the discussion and conclusions.

Also from the introduction one may quote the following succinct definition of the strategy and limitations of the excavations on the tell: 'The excavations therefore consisted of a series of soundings designed to establish the history of the site rather than to provide a large exposure of the structures of any one period'. The format of the publication takes the excavation areas in sequence and describes the stratigraphy and the structures within each exposure from bottom to top. Fortunately each chapter (each reporting a trench) is begun with a summary, which is quite essential. Some decision has to be taken in regard to general method of treatment, but, until the discussion and conclusions appear in Volume IV, where the whole site will presumably be considered period by period, it is extremely difficult to study the remains of any single period. The tables of levels and phases in each trench, and the stages and periods which they represent, together with the page references, are invaluable; and they lurk under the guise of Appendix D, beginning on p. 505.

Within each trench each stratigraphic deposit is designated with a lower case Roman numeral, and the strata are grouped logically into phases, each phase representing a piece of structural history, for example the construction of a building or room, its subsequent use and refloorings, each distinguished by a postscript letter after the Roman numeral, and finally the abandonment or destruction stratum. In fact, there is already a stage of interpretation introduced into the observation in that the strata or units are not simply numbered in sequence or as found, but are related to the 'foundation' stratum of each phase. Phases are then themselves grouped into stages, and 'usually a new stage is given when there is a complete change in plan'. Finally stages are grouped within the broader archaeological periods with their period/culture names. To my mind it is strange that so much care

was given to devising such a meticulous recording system which did not confuse cultural period with stratigraphy, and yet the nature of the deposits in the stratigraphy is defined in the drawings in terms of the phases or stages to which the strata are assigned; this information is repeated in the margins of most section drawings, and one is left with no graphic description of the physical nature of the deposits. It is also apparent from the text that the decision not to number rooms or buildings, which in one sense admittedly would have further complicated the report, has led to inelegant and imprecise circumlocution in the text. In fact lengths of wall were numbered, but particularly where the walls were poorly preserved there may be more than four numbers to the fragments of wall which define a room. In these circumstances reference to a room or a building can become both tortuous and unclear. Having recently spent a good deal of time working through the 'Ai reports of Callaway and his colleagues, I suspect that their labelling and identification, even more complex as it is, yields in the end (which is in the publication itself and the reader's ability to use and understand it) a more comprehensive and comprehensible result: Jericho was dug in the 1950s and 'Ai in the 1960s, and there we see progress, which is far from being inevitable, especially where excavation and recording technique have been concerned.

The section drawings have been reduced as far as possible in the competing interests of clarity and economy; but it is rather strange that the drawings have been made up on very many oddly sized and over-sized sheets, which are fold-outs in two directions. These must have cost a great deal to produce, fold and bind in, and they make the handling of the book rather difficult; inevitably, in library use these will become mal-folded and torn at the binding. The half-tones are many, clear and excellently reproduced. The text is beautifully set and printed.

Volume III at Jericho includes Cornwall's full report on the burials found in the tell, and here are the detailed accounts of the pre-pottery neolithic burials and skull-cult. Following naturally from that report is the physical anthropological report on the human remains. Though no specialist I wonder at the length of this report with its extensive systematic forays into the comparative material from Khoirkitia in Cyprus, Byblos and many places further away in space (Sweden) and time (medieval). The other specialist report is that from R. Burleigh on the radiocarbon dates; its text is surprisingly brief, not even mentioning by what tables or graphs the third millennium Early Bronze Age dates were calibrated. The tables of dates, however, bring together not only all the dates by various laboratories of the late 1950s and early 1960s, but also a series of newer determinations from the British Museum laboratory. These dates confirm that the mesolithic was in the late tenth millennium bc, the PPN-A in the eighth rather than the ninth, and the PPN-B in the early seventh millennium. Two new dates for 'Proto-Urban' Tomb A94, one on the same sample which had previously yielded a raw figure of 3260 ± 110 bc, now place the tomb in the third quarter of the fourth millennium BC in calendar years. The series of EB radiocarbon dates published in *Radiocarbon*, 19 (1977), are here republished and with a calibrated approximation alongside: with one exception (2480 BC) the seven dates lie in the general range 2950–2700 BC, marking the latest EB occupation of the site before Kenyon's EB-MB period. The chronological discussion in Volume IV will be worth waiting for.

As it happens, as Secretary of a society which produces a large annual volume heavily subsidised to print the reports of Scottish excavations done under rescue funding, I am concerned at the bulk of many such reports and in particular the cost of producing them. At the same time, having been a rescue excavator, I also produce some (by British prehistoric standards) fairly heavy reports for publication. As reviewer, as Secretary of a learned society, and as author, I wonder for whom these massive reports are produced, and I suspect that no readership is in our minds so much as what we feel is our duty to science. With the publication of the Jericho excavations the die is cast, and we can only hope that Volume IV, which is so vital to the understanding of Volume III, will appear soon, while some of our libraries can still be persuaded to buy it. In general, however, the weight and cost of the Jericho report persuade this reviewer that there must be another way, if not with microfiche then with micro-chips.

TREVOR WATKINS

CALLAWAY, J. A. *The Early Bronze Age citadel and lower city at Ai (et-Tell): a report of the joint archaeological expedition to Ai (et-Tell): no. 2* (American Schools of Oriental Research Excavation Reports). Cambridge, Mass., American Schools of Oriental Research, 1980. 295 pp., 155 figs. \$20.00 (pre-publication).

BOOK REVIEWS

For reasons of space it is not possible to present a detailed discussion of the large corpus of artefacts contained in this site report, or the comparative material; consequently, this review will be confined to discussion of three aspects of this publication: the quality of the production of the volume as a book, the quality of the evidence contained, and the quality of the analysis of the evidence.

To take the first point, there are a great many praiseworthy points about the production of the volume, not least of which is its prompt appearance, in spite of unavoidable delays at the printers, which were obviously beyond the author's control. The size of the volume is ideal, neither too large, nor too small. The figures, whether line drawings or photographs, are reproduced clearly, and at an appropriate scale. The innovative method of producing the larger plans on a double-page spread, but with sufficiently wide centre margins so that nothing is lost in the junction of the pages cannot be praised highly enough: the long-term advantages of this format over large folding plans (separate or attached) should be obvious to all. There are mercifully few typographical errors, although a number of those which have slipped through could cause some confusion for the unwary reader:

- p. 56: 2nd column, line 2 – for 'Area C IX' read 'Area C IV'
- p. 119: last line – for '(fig. 32)' read '(fig. 80)'
- p. 121: line 6 – for 'Wall D' read 'Wall O'
- line 8 – for '(fig. 60)' read '(fig. 80)'
- p. 154: 1st column, line 3 – for '(fig. 99)' read '(fig. 98)'
- line 13 – for '(fig. 99)' read '(fig. 98)'
- p. 157: 1st column, 2nd paragraph, line 6 – for 'C VIII' read 'C VII'
- p. 158: The Phase VI Houses at Site G, line 3 – for '(fig. 19)' read '(fig. 31)'
- p. 191: 1st column, 2nd paragraph, line 8 – for '(fig. 12)' read '(fig. 120)'

As regards the quality of the evidence, the system of observation, excavation, and recording used has been discussed in detail in the first volume of this series of reports (Callaway, J. A., *The Early Bronze Age sanctuary at Ai (et-Tell)*, London, 1972), and meets the most rigorous modern standards. Only one point need be raised here: in the discussion of the technical terms used in the report the term 'sub-layer', which appears frequently in the present report, does not occur. The 'sub-layer' number appears to be the same as the 'Field No.' in the 'Table of Layers and Pottery Information'. If the 'layer' is a homogeneous unit of material, as defined in the list of technical terms, it would be useful if some definition of the 'sub-layer' could be given, so that the reader of this report might know on what grounds a given 'layer' had been so divided.

It may seem something of a quibble to say so, but in view of the value of the information revealed by the replanning of House 238, 195B, 198, it seems a shame that more of the Lower City areas excavated by Marquet-Krause could not have been cleared and re-planned. Also, in view of the limited number of Early Bronze Age sites unencumbered by later occupations, it seems a great pity that it was not deemed practical to expose a wider horizontal area, to gain information on house and site planning, as well as cultural/artefactual sequence.

Finally, we come to the assessment of the quality of the analysis of the artefactual evidence recovered in the excavation. Considering the admirable quality and quantity of the primary data with which Callaway has provided us, it is a great pity that his analysis of the excavated material has been pitched on such an elementary level. He has made a freer use of the 'Invasion Hypothesis' for the explanation of cultural phenomena than almost any scholar with whose work the present writer has come in contact.

1. A group of northern invaders establishes the 'Pre-Urban' settlement.
2. A group of northern invaders conquers and rules the 'Urban B' settlement.
3. A group of southern invaders (Egyptians) constructs the 'Urban A' public buildings.
4. A group of northern invaders (Khirbet Kerak) conquers and rules the 'Urban A¹' settlement.
5. A group of southern invaders (Egyptians) destroys the site at the end of 'Urban A¹'.

It is difficult to take this completely seriously; as a single-factor explanation it contains both too few and too simple causes to constitute an adequate explanation of either the history of the specific site, or of Early Bronze Age Palestine as a whole. No reasons are given to explain why the Egyptian and Khirbet Kerak items, which form only a minute percentage of the total assemblage,

could not have been peacefully acquired in the course of normal trade relations. There is no presentation, or, apparently, consideration, of environmental evidence, from this or any other site, nor of the question, paramount in much of modern anthropological thinking, of the population dynamics of the site, the balance between numbers of people and productive capacity of the Early Bronze Age technology in the specific environmental setting of Early Bronze Age Palestine in general, and the region surrounding Ai in particular.

To take but one example of where consideration of the artefacts from an economic point of view might lead, it is possible that, if the hypothesis that the Early Bronze Age III saw the beginnings of a shift from an agriculturally-based economy to a pastorally based economy in Early Bronze Age IV is correct, the apparent popularity of the sheep as a decorative motif in the latter part of the Early Bronze Age III, discussed by Callaway on pages 193–4 of the volume under review, might be a reflection of this fact. Clearly, much more could be done with the wealth of material produced by the excavations at Ai than has been attempted in the present volume; it is to be hoped that future volumes in this series will include more specialist studies, particularly of environmental evidence, and that an analysis of the material which takes greater account of modern thinking in anthropology concerning both the causes and mechanisms of cultural change, and the variety of forms of cultural interaction other than invasion and conquest will be attempted.

In spite of the criticism voiced above, a balanced view of this volume must see it as constituting one of the most important site reports for the study of Early Bronze Age Palestine yet to appear.

RUPERT CHAPMAN

BUCHANAN, Briggs. *Early Near Eastern Seals in the Yale Babylonian collection*. Edited by Ula Kasten. Introduction and seal inscriptions by William W. Hallo. New Haven, Yale University Press, 1981. 498 pp., unnumbered plates of 1,286 seals. £45.50.

This long awaited catalogue, mentioned by the author in numerous articles since 1960, has at last been published. Understandably, the delay may have been the result of the untimely death of the author.

The arrangement of the catalogue follows a chronological order from the prehistoric period right up to the end of the Old Babylonian era and covers most of the geographical area of the Ancient Near East. The seals from Mesopotamia proper are catalogued first while the peripheral seals are dealt with separately within their chronological phase. The seals of each period, where applicable, have been arranged according to subject or style. The inscriptions which are studied by W. Hallo form a separate chapter. There is a concordance of Museum numbers, as the Yale Babylonian Collection of seals comprises three rich collections: the Newell, already published (H. H. von der Osten, *Ancient Oriental seals in the collection of Mrs. Edward T. Newell*, Chicago, 1934), the James B. Nies and the collection of Yale University.

Apart from the fact that this is a very rich and important collection, the catalogue is of great help to the student of Near Eastern glyptic. The author includes references to published parallels to seals of special significance. The inclusion of a bibliography to seals has already been attempted, but only in smaller catalogues appearing in archaeological journals. Another important contribution in this catalogue is the inclusion of ancient seal impressions which has been so far greatly neglected by many scholars. Impressions on cuneiform tablets are of equal importance to the history of the development of glyptic art as actual seals themselves. In fact, an impression can sometimes be of considerable help if it is impressed on a tablet dated to a particular king. An assemblage of dated impressions would then provide a clear interpretation of the development of the styles of a particular period, e.g. Ur III or Old Babylonian.

The arrangement of the photographs in plates which are placed on opposite pages to their corresponding description deserves to be singled out as it saves the reader a great deal of otherwise wasted time. It is unfortunate that the inscriptions of the seals which could have been included here have been placed separately at the end. It is also a pity that the quality of many of the reproductions, particularly the impressions from the tablets, cannot be commended especially as they are not aided by drawings. Anyone who has tried to photograph seal impressions on tablets appreciates the difficulties involved, thus the drawings of the impressions is an essential part of any catalogue.

BOOK REVIEWS

As already mentioned the catalogue and the description of each seal is adequate. However, the introduction and discussion of each group is unsatisfactory. These are both brief and mention only the outstanding features of the seals in the collection with no explanation to their special interest. The author discusses the Prehistoric stamp seals in general and avoids precise dating or possible provenance of the individual seals. Furthermore, he gives diverse comparative examples from different sites, e.g. No. 113. No reasons are given for the allocation to style or the dating of many of the seals, e.g. why the Brocade Style is in the Jamdat Nasr period, or why should the Late Old Babylonian seals be divided into 'Transition to Schematic Representation' and 'Stylized Design'. For example, I fail to distinguish between No. 1027 of the former and either 1034 or 1077 of the latter group.

Yet, despite many of the shortcomings this is an essential volume, providing new and valuable material to the study of Ancient Near Eastern seals.

LAMIA AL-GAILANI

DAVIES, G. I. *The way of the wilderness: a geographical study of the wilderness itineraries in the Old Testament*. (Society for Old Testament Studies Monograph Series 5). Cambridge, Cambridge University Press, 1979. xii + 138 pp., 3 maps. £11.50.

Identifying the route of the Exodus is one of the most intractable problems facing either Biblical studies or archaeology in Palestine, given the complexity of the textual evidence and the effective absence of any archaeological remains. In view of these facts, and the vast amount of effort devoted to the resolution of these difficulties over the past hundred years alone, it is not to be hoped that a definitive identification of more than a handful of the locations mentioned in the wilderness itineraries will ever be arrived at. Nevertheless, the problem is an important one, and this new consideration of it should be warmly welcomed.

The rigorous methodology which underlies the studies of the various ancient and Medieval traditional identifications, and the careful treatment of the development of these traditions and of their modern counterparts in the past century's academic treatment of the subject have rendered both the complexities of the problem, and the advances made, very clear.

The book has a few outstanding weaknesses, perhaps the most unfortunate of which is its price, which is high even by present standards. The provision of translations of the various lengthy passages of Hebrew, Aramaic, and Greek would have rendered the book easier reading for the non-linguist archaeologist for whom the problems discussed are also important and, doubtless, the specialist Medieval Arabic student will regret the lack of the original Arabic texts. It seems a great pity, also that the detailed and very welcome study of the known modern routes in the Sinai Peninsula with their assigned numbers is not reflected in the maps. Finally, the provision of no less than 25 pages of discursive footnotes, many of which might easily have been included in the text, tends to break up the flow of one's thought and result in a tedious, continuous flipping from front to back of the book.

In sum, with its welcome treatment of the latest textual and archaeological findings, as well as its coverage of the older work on the subject, this book should prove a useful addition to the bibliography on the subject.

RUPERT CHAPMAN

BIKAI, P. M. *The pottery of Tyre*. Warminster, Aris & Phillips, 1978. ix + 92 pp., 95 pls. £24.00.

The rather grand title of this book is somewhat misleading, for the 'pottery of Tyre' actually comes from a very small area, at maximum 15 × 10 m, and becoming even more reduced (5 × 5 m) at the lower levels. This is a very important point to make at the outset, for any conclusions on the occupational history of the site must be tempered with consideration for the extremely limited scale of excavation. The sounding, undertaken during 1973 and 1974 as a part of the Tyre project, was executed in an area to the north of the Crusader Church excavation, and this volume examines the pottery from strata XXVII to I, covering the period EBII (c. 2850 BC) to Iron II (c. 700 BC). Stratum I was overlain with remains of the Roman period, the levelling for the construction of whose buildings had effectively removed all traces of the Hellenistic and Persian periods, known however from previous excavation to be well represented at Tyre. These post-Iron Age levels are to be reported elsewhere and Bikai makes only passing reference to them. The organisation of this book is confusing.

Chapter 1 is straightforward enough and deals with the architecture and stratigraphy of the sounding in a clearly presented fashion, taking each stratum in turn, from the earliest (XXVII) to the latest (I). References to the well-drawn plans, which appear at the back of the book, are easily followed. On the other hand, very little reference is made to the sections, which in a discussion of this sort are of course of prime concern: indeed it is almost as if they were added to the volume as an afterthought without any adjustment to the text.

The logical approach established in the first chapter disappears with the second, for the discussion of the pottery corpus begins with the Late Bronze and Iron Age material from strata XVIII to I, the earlier pottery being left until chapter 4. Stratum XVIII was a deep layer of soft sand, and because of the difficulty in preventing the baulks from slipping, the original 15 × 10 m sounding was reduced to 5 × 5 m in order to excavate the strata below (XIX–XXVII). Consequently, the volume of pottery recovered from the upper strata was much greater than that from the lower, and the method chosen for dealing with this later material (strata XVIII–I) was by means of a type series. Each type ('jug', 'deep bowl', 'store jar', etc.), is described, the number of occurrences noted, references made to the plates and parallels cited wherever possible. Following these descriptions, the types are discussed in relation to the stratigraphy, utilising frequency and distribution charts rather in the manner of Woolley's *Alalakh* report. Finally comes the most useful part of the chapter; a stratum by stratum breakdown of the pottery types. It must be said that the overall impression is that this section of the book is grossly overworked and at times frankly pedantic in its approach, and very clearly just the sort of thing required for a PhD thesis (which is exactly how this book started its life) but which could have been considerably pruned for publication. The value of presenting a type series of this sort must in any case seriously be questioned, especially when the result, as here, is totally confusing. When the plates themselves are consulted, they are seen to be arranged not by type but by stratum! Since, however, only one or two examples of a particular type may be illustrated, any one plate does not necessarily represent the entire repertoire from that stratum. This is a great drawback for those scholars, of which there will be very many, who wish to simply turn to a certain plate and gain an impression of the pottery from a particular stratum. They will certainly find some pottery of that stratum, but types which happen to occur also in other strata may be illustrated elsewhere on plates representing those strata. Furthermore, there seems to have been a singular lack of thought in deciding which types to illustrate and where. To give an example of this; the most common cooking pot of strata X–XI, according to the frequency chart on p. 51, is that of type 6. The type is illustrated on plates XX and XXXIII, corresponding to strata VIII–IX and XII respectively, yet on plates XXIII–XXX, which show pottery from strata X–XI, type 6 is not illustrated at all. Another point worth making is that having accepted to use a type series in order to avoid repetition, what on earth is the point of illustrating 17 all but identical store jars, all of type 5 and all from stratum II (plates II–III)? On the whole, the type series method should be avoided: it is cumbersome, confusing and always subjectively interpretative. It is far better, even at the expense of repetition, to examine each stratum in turn and define, describe and illustrate all of the differentiated pottery types from that stratum.

Chapter 3 is a well-written and very sensible discussion of the chronology of strata XVIII–I based on the preceding pottery analysis and relying heavily on the imported Greek and Cypriote wares. It is only after this chapter that we return to the pottery from the earlier periods (strata XXVII–XIX). The volume of this material, coming from the more restricted sounding, was not large enough (thankfully) to warrant incorporation into the type series, and so the pottery is discussed form by form, stratum by stratum. The material from these early strata is in many ways the most interesting, and covers the period from late EBII (c. 2850 BC) through EBIV (c. 2000 BC). Not unexpectedly, parallels throughout are mostly with northern Palestine rather than with inland Syria, but the most significant feature is the unbroken nature of the sequence, for Bikai's excavation has substantiated the view that settled conditions, perhaps even fully urban, persisted on the coast throughout the period during which most of Palestine, especially in the south, witnessed a phase of recession leading to a breakdown of urbanism and the establishment of a semi-nomadic mode of existence in the EBIV (Kenyon's EB–MB). Even this is not totally unexpected, since it should be remembered that Tyre, together with Byblos, is one of the very few Syro-Palestinian cities mentioned in the first, 'Berlin' group of execration texts dating to the EBIV period, whereas the majority refer to general localities or tribal groups, well reflecting the semi-nomadic aspect of this period in Palestine.

Chapter 5 discusses the contribution made by the excavation to elucidating the history of Tyre. The earliest pottery found dates to around 2850 BC, and Bikai cautiously points out that this

date accords remarkably well with the Tyrian tradition, as preserved by Herodotus, which sets the foundation of the city at around 2750 BC. Bikai is well to be cautious, for on the basis of a 5 × 5 m sounding nothing too positive can be said about the foundation of Tyre: it is entirely possible that a sounding in another location may reveal extensive remains of an EBI or Chalcolithic settlement. Perhaps the most intriguing aspect of the whole sequence as perceived through the excavation is the apparent break in occupation between strata XIX and XVII, represented by a layer of soft sand, sterile except for three graves of the Late Bronze Age cut down from above. This gap of some 400 years, and corresponding to the whole of the Middle Bronze Age, is difficult to reconcile with the historical data, for Tyre is amongst the cities mentioned in the second 'Brussels' group of execration texts which must refer to a time within the MBIIA period (c. 2000–1750 BC). Bikai is very much aware of the problem and suggests two rationalisations: firstly that since both Posener and Albright qualify their identification of Surrûyu as Tyre with a question mark, neither scholar was certain. In fact, Albright merely quotes Posener's identification and passes on the latter's caution. In any event, the identification cannot seriously be doubted, for if Surrûyu is not in fact Tyre then it must refer to a city with an all but identical name! Bikai's second explanation is equally improbable: that the Egyptian compiler was relying on earlier lists of enemies whilst writing his curses, blissfully unaware that Tyre was no longer occupied, seems just a little naive. The problem of this gap must remain for the moment unresolved to await the evidence of more extensive excavation. For, as stated previously, a 5 × 5 m sounding is much too small an area on which to base any conclusions as to occupational history. Again, another sounding in a different area may well bring to light the Tyre of the Middle Bronze Age. Certainly this would help to account for a number of 'stray finds' from Tyre clearly belonging to the period in question such as the 'type-fossil' MBIIA duck-bill axe published by Ronzevalle (*Mélanges de l'Université Saint-Joseph*, VII, pl. XX: 1). Bikai herself suggests that the deposit overlying stratum XIX at the base of the sand layer might have been produced by the action of a tidal wave: after such an experience, the inhabitants may well have selected a new location in which to settle. Otherwise, the excavation has not added new information to our knowledge of the history of Tyre, but in this very useful chapter Bikai succinctly and intelligently relates the known historical events to the discerned stratigraphy.

Altogether, this is not a bad book, and if the review sounds somewhat negative, it should not disguise the fact that there is some very interesting material published here. Bikai's written style is good, clear and fluent and has the property, so rare in technical archaeological reports, of being able to sustain the interest of the reader. With more preparation the volume of text could have been considerably reduced, and a consequent reduction in the price of the book would have ensured that this valuable material had reached a wider audience.

JONATHAN N. TUBB

HANSON, Richard S. *Tyrian influence in the Upper Galilee*. Cambridge, Mass., American Schools of Oriental Research, 1980. 89 pp., 5 figs., 16 pls. \$8.00 (paperback).

The first point one must make about this monograph is that its title is seriously misleading. A more appropriate title would be *Tyrian coinage: a study based on examples from the Upper Galilee*.

Having said this, it is an interesting study, even for the non-numismatist, and may prove to be of great utility in a number of ways. In addition to its direct contribution to our knowledge of the history of Tyrian coinage as such, the distribution of the coins within the various sites should, as the archaeological coverage of the area increases, enable increasingly sophisticated spatial analysis of the area of Tyrian economic influence from the period of Persian domination onward. The value of this coin evidence for refining the ceramic type seriation, mentioned in the introduction, will also be very great, although in view of the long life of some of the coins in circulation revealed in the study (p. 51) great caution will obviously need to be exercised.

While recognising that this might lead to some limited duplication of more detailed publications due to appear later, it does seem a great shame that more could not have been said about the kinds of contexts from which these coins have come; as matters stand we know nothing at all about this, indeed, Upper Galilee is given no more than a passing mention. An interesting question is raised

BOOK REVIEWS

by the apparent importance of Tyre in the economy of Upper Galilee in this period, namely, is this pattern paralleled in the earlier periods and for other Levantine port-cities?

In sum, this is a useful, if limited, contribution, whose implications should be further explored and analysed in future volumes of the Meiron excavation project series.

RUPERT CHAPMAN

HOPKINS, C. *The discovery of Dura Europos*. Edited by Bernard Goldman. New Haven and London, Yale University Press, 1979. xxiv + 309 pp., illus. £12.60.

The discovery of Dura Europos is a useful and concise summary of the major finds at Dura and their contribution to our understanding of the complex art of the Greco-Roman and Parthian periods in the Middle East. Dura was founded by Seleucus I c. 300 BC, conquered by the Parthians around 113 BC and held as their most western frontier post on the Euphrates until AD 164. It then became a Roman colony until its final destruction by the Sasanians in AD 256.

Although an informative description of the archaeological discoveries combined with precise discussions of the historical background form the framework and most important aspect of this book, it is hardly a dry account of excavations at Dura Europos. There are amusing anecdotes about the people involved, and a fascinating description of the contemporary political situation in the Near East makes the reader close his eyes and imagine the turbulent time between the two world wars. In spite of the unstable political situation, excavations in Syria flourished under the French mandate. This is best summarised in the first three chapters which deal with the discovery of the paintings at Dura by British troops in 1920, Franz Cumont's excavations in 1922, and Michael Rostovtzeff's involvement which resulted in the French-American excavations of 1928-37 with Maurice Pillet as field director. The author of this book, Clark Hopkins, joined the team in 1928 and took over as field director in 1931. The last two seasons were directed by Frank Brown. The difficulties of the early seasons are amusingly described. When Cumont, the Belgian scholar, started working at Dura, he was provided with a group of soldiers to start the excavations, with no tools or trained assistants. He had to act 'as director, archaeologist, epigraphist, photographer and carpenter'. Only a decade later, though, Hopkins was excavating with 300 workmen and helped by 'a veteran and able staff'.

The reproduction of old black and white photographs of the native Arabs, the desert police and the expedition house at Dura bring the excavation vividly to life. But most interesting are the photographs of the staff, including Hopkins himself, Pillet, Rostovtzeff, Cumont, du Mesnil du Buisson and Seyrig which help to give an impression of these veteran and distinguished scholars whose names have already become myths.

Two major chapters, the seventh and the ninth, deal with one of the most significant and fascinating aspects of the discoveries at Dura: the paintings of the Christian Baptistery and the Synagogue respectively. Here, as in other parts of the book where he discusses the art of Dura, Hopkins' main intention is to point out the appearance and importance of the different traditions: a combination and co-existence of Greco-Roman, Oriental-Mesopotamian and Partho-Iranian motives. The paintings of the synagogue are carefully described and discussed panel by panel. Detailed photographs of some of them within the text and a schematic arrangement of the wall paintings on p. 266 help the reader to follow the discussion. The number of panels follows that adopted by C. H. Kraeling in his final report on the synagogue (*The excavations at Dura Europos, Final Reports*, VIII, Part I, New Haven, 1956), thus enabling the reader to make easy cross-reference. The paintings are carefully described in connection with the Old Testament which provides the variety of themes, and there is a thorough examination of Jewish history during the 3rd century AD and of historical events at Dura itself. The interpretations of scholars such as Gutman and Kraeling are usually mentioned with reference to the excellent and up-to-date bibliography which anyone interested in Dura Europos will find indispensable.

But one should not forget to give as much credit to the editor of this book, Bernard Goldman, as to the author himself. When Clark Hopkins died in 1976 the text was partially written and revised, but there was also a major portion in preliminary draft, sometimes consisting only of a list of items to be mentioned. The story of the last campaign, for example, was reconstructed by Goldman from the letters sent by Hopkins to his wife, Susan, and from copies of three field reports which she had saved.

VESTA CURTIS

BOOK REVIEWS

RAGETTE, Friedrich. *Baalbeck*. London, Chatto & Windus, 1980. 128 pp., illus. £9.50.

A text for the interested layman rather than the research student, this book nevertheless does contain throughout, and in Chapter 10 particularly, detailed and informative architectural studies of the various temples and their precincts; the author is Professor of Architecture at the American University of Beirut. Overall the book is of a general nature, which would make excellent reading for any non-specialist preparing for a visit to Baalbeck, and equally for a student needing an introduction to the site.

The first chapter places Baalbeck in its topographical and strategic setting and the penultimate one tells of its rediscovery by Europeans from the 16th to the 19th centuries AD. The next chapters summarise the history of the area and its religion, both in the pre- and in the post-Roman periods, but concentrate in the main on the better-known Imperial era when the temples were built, and for which the site is primarily famous. An attempt is made to assess the relationship of the architecture and the rites of these temples, influenced as they were by Near Eastern traditions and beliefs, to those of the heartlands of the Roman Empire. This is quite illuminating, but must necessarily be brief.

The book also outlines some of the most recent findings by Lebanese archaeologists and restorers, including the location of the ancient tell of the 2nd millennium BC beneath the main temple, that of Jupiter Heliopolitan. The author cites the belief of Professor Kalayan, the engineer in charge of restoration, that the origin of the sanctity of the site in remote antiquity may lie in the presence of a deep crevice in the bedrock of what is now the courtyard of the Temple of Jupiter, and theorises on the evolution of the cult and its buildings from earliest times until the syncretism with the head of the Roman pantheon. He also describes the Christian era when a basilica was built partly over that same courtyard. One chapter (no. 7) is devoted to the little that is known of the Roman town apart from the Temples, and points out the difficulties of excavating what is still an inhabited area, while the last chapter (no. 10) gives an account of restoration work recently carried out in the sacred precinct.

The weakest part of the book seems to me to be some of the archaeological and theological assumptions in Chapters 2 and 3; but these should be forgiven a scholar who is an architect rather than an historian, and especially in a book which sets out to be no more than a brief outline of the subject. Professor Ragette's own expertise shows to advantage in his choice of black and white photographs and line drawings which copiously and helpfully illustrate his text.

ROBERTA L. HARRIS

BROWNING, Iain. *Palmyra*. London, Chatto & Windus, 1979. 223 pp., 151 figs., 4 pls., 4 maps. £8.95.

This is a survey of the history, discovery and monuments of the famous caravan city which would be a useful companion for any visitor to the site. Chapter 5 is the largest section of the book, and this is arranged in parts to facilitate a walk around the ruins. The author has paid considerable attention to architectural and constructional details which would certainly help the tourist to understand and appreciate the site. However, the book would not seem to have much appeal beyond this, as it was not conceived on a level to aid deeper research into either the history of the architecture of the region. It must therefore remain an introductory text for the general reader and the lucky tourist. In fact that reader who has not had the opportunity of visiting Palmyra might finish the book with an overwhelming sense of claustrophobia among the endless colonnaded boulevards with which the citizens tried to shut out the desert beyond their city.

In its heyday in the 2nd and 3rd centuries AD the position of Palmyra, while politically within the Roman Empire, was that of a buffer between the hostile worlds of Rome and Parthia, and its wealth was won from the caravan trade by which the merchants ferried luxuries from one power bloc to the other. That wealth was used with enormous civic pride to embellish the city, and to create in the Syrian steppe the ancient equivalent of a Hollywood set for a Roman epic. That, at least, is the overwhelming impression which Mr Browning's book makes on the reader; and it is an impression which is hardly mitigated by his occasional flights of literary fancy (cf. p. 138 for a good example). In Chapter 4 there is an excursus on the influence of Palmyrene architecture on 18th century English country houses, which, however interesting and expert, would seem to be out of place here. The many

BOOK REVIEWS

line-drawing reconstructions of buildings, interiors, etc., are not all, as the author himself admits, solidly based on excavational research: much of the city remains undug.

Some of the photographs, most of which are in black and white, are of a high standard, but unfortunately not uniformly so: neither is the architectural glossary at the end particularly comprehensive. In spite of the romantic aura surrounding the city's name and its association with Zenobia, and in spite of the author's own obvious enthusiasm, this book makes Palmyra out to be rather a pedestrian ruin.

ROBERTA L. HARRIS

SETON-WILLIAMS, M. V. *Les trésors de Babylone*. Paris, Editions Princesse, 1981. 215 pp., 170 pls. £16.80.

This lavishly produced volume is in a different class from the usual run of expensively priced books which attempt to give popular accounts of ancient civilisations by means of coloured photographs of a number of often unrelated objects with the minimum of explanatory text. Here the text includes a detailed survey of the geological and ecological background of the earliest agricultural communities in Iraq with sections on the main prehistoric sites from the earliest periods and chapters on the Uruk period, Early Dynastic Sumer, the Akkadian and Neo-Sumerian periods, ancient Babylonia, Assyria and the Neo-Babylonian period ending with the Parthians. The illustrations are an essential accompaniment to the text with captions which are informative not only for details of manufacture and technique but also for the archaeological context of each object. A useful bibliography is included after each section of the illustrations whose standard with only a few exceptions is extremely good. Some of the gold work in the Iraq Museum has not been previously photographed and for the first time we have a colour print of the whole of the magnificent necklace of Abbasashti so that the different veined agate beads can be studied in detail.

Let us hope that an English edition will soon be available for students, and that it will include a list of illustrations, surprisingly omitted from this edition. On p. 112 for Bucharan read Buchanan.

K. R. MAXWELL-HYSLOP

BLAKELY, J. A. and TOOMBS, L. E. *The Tell el-Hesi field manual: The joint archaeological expedition to Tell el-Hesi*. Vol. I. Cambridge, Mass., American Schools of Oriental Research, 1980. xix + 134 pp., 39 illus. \$12.00.

This volume is a welcome addition to the recent list of publications of techniques of observation, excavation, and recording accompanying excavation reports on Near Eastern sites. As with the other such books it offers the only means by which the reader of the site report can make an informed judgement of the standards the excavators have set for themselves in the methodology they have adopted for the recovery and recording of the archaeological information from the site excavated. As it is this information which is vital for the analysis and/or re-analysis of the archaeological information recovered on the site, while the details of the staff organisation and finds processing procedures are of purely secondary interest to the reader of the report, it is on the recording system with its related systems of observation and excavation, and in particular the vital terminology and definitions used, that this review will concentrate.

In the chapter on the 'General Principles of Excavation' it is stated that 'The true stratigraphic sequence within a site is given by the relationships of the soil layers to each other', and 'The excavator attempts to remove the soil layers in the reverse order to that in which they were deposited' (p. 18). While it is true that these two sentences contain in a very condensed form the essence of a scientific methodology of excavation and recording, it does seem a great pity that in a manual intended, at least in part, for the instruction of the volunteers who carried out the actual work of digging, more emphasis was not placed on the various laws governing the analysis of the stratigraphy of any site, in particular the Law of Superposition, which would clarify, for the uninitiated, both the nature and the significance of the relationship referred to.

More serious, however, is the confusion inherent in the definition given for the basic recording unit used, termed (in the American tradition) a 'locus'. In the first place the term 'locus' means something different to almost everyone who uses it, which is not particularly productive of clarity. In the second place, the locus as here defined consists of 'any layer or feature which appears in an area' (p. 8). While the examples of loci given are wholly acceptable ('soil layers, walls, surfaces, or pits'), the qualifying 'or features' included in the definition raises the possibility of loci which consist of entities 'such as a room, a courtyard, a pit, or an industrial installation' (p. 8). If, as seems probable, loci and features were intended to be entities of different hierarchical orders, it would seem far better to have made this quite clear in the definitions.

The most serious methodological weakness in the system of observation, excavation, and recording used at Tell el-Hesi is contained in the statement that 'It must be added that most loci are real structural or architectural features, but that artificial loci also exist' (p. 8). The use of the same term used for such empirical entities as soil layers, walls, etc., for an *a priori* entity such as a probe means that the locus itself has no fixed epistemological status or definition. Why should not a probe be indicated on the site plans, given a number framed by a co-ordinate system (such as that used at Gibeon, Hazor, and Samaria) and the identified stratigraphic units within it be recorded as separate examples of the basic recording unit, with a note as to the degree of probability of their containing intrusive material? If a uniform abstract co-ordinate system (site-grid) covering the whole of the site had been imposed from the beginning, and the area grids (but not more than a handful of main sections) aligned to this, such a procedure would be the essence of simplicity, and much preferable to the confusion introduced by the one outlined in the field manual. This would, of course, require extensive use of subsidiary sections to illustrate the stratigraphy of 'features' as defined in the Tell el-Hesi manual.

It should be noted that Tell el-Hesi 'areas' are laid out as 5 × 5 m squares with 1 m wide baulks between. This is the 'grid system' advocated by Wheeler in *Archaeology from the earth* (Wheeler, 1954, pp. 82–6). This tactical system has been much criticised within British archaeological circles (and elsewhere!), and has been largely abandoned by archaeologists working in Britain in favour of open-area excavations without permanent baulks, various systems being used for obtaining sections on fixed lines, most of which involved drawing shallow portions of sections cut along surveyed lines as the deposits concerned are excavated. The problems of spoil-removal logistics on such excavations are easily handled by the use of wooden or metal trackways which can even be raised on concrete blocks if this is felt necessary, and which can be moved about as required.

In view of the critical importance of the work of the various scientific specialists involved in modern excavations, it seems a great pity that the chapter on 'Specialists' is so short and general. Surely more space could profitably have been devoted to their work. In particular, given the importance of proper procedure in the taking of samples for scientific analysis, it seems strange that no instructions are included as to what samples in what quantities are likely to be required for what sorts of analyses, and which of these may be collected, by what methods, by volunteers, area or field supervisors, and which should be collected by the specialist concerned and why.

Finally, it should be noted that the practice of soaking all potsherds for 24 hours before washing is unduly risky. If the sherds treated in this fashion happen to be either unfired, poorly fired, or have suffered damage due to soil conditions (high acidity, for example), the soaking-pail is likely to be filled with mud, not sherds, at the end of the specified period.

In conclusion, then, the *Tell el-Hesi Field Manual* gives us an extremely valuable set of insights into the organisation of the expedition and into the theoretical thinking of the excavators on the problems of observation, excavation, and recording, and on the ways in which that thinking has been put into operation. Excavation has been referred to as an 'unrepeatable experiment' (Barker, *Techniques of archaeological excavation*, Batsford, London, 1977, p. 12). In those disciplines in which repetition of experiments is possible, full disclosure of the methods used in the experiment and the hypotheses and theoretical principles underlying those methods is considered essential for the validation of the experiment itself and the conclusions drawn from it. The publication of this field manual shows the increasing and welcome recognition that this standard of critical evaluation must be applied to archaeological 'experiments' also.

RUPERT CHAPMAN

BOOK REVIEWS

GOFF, Clare. *An archaeologist in the making: six seasons in Iran*. London, Constable, 1980. 284 pp., illus. £9.95.

Whereas Clark Hopkins' book reviewed on p. 00 is a useful account of an excavation and makes an important contribution to archaeological research, this is a popular book which does not pretend to do more than give an amusing memoir by Clare Goff on her work in Iran. Her main intention in writing the book was to answer some of the questions posed by every layman interested in this field: how is a site found, how is the dig financed, how is a crew assembled and – the most difficult task of all – how does a foreigner deal and communicate with the local community whose language and customs are often unfamiliar. She certainly gives a vivid description of all the problems of financing and most of all running a successful dig. But perhaps it is her strong involvement and interest in the human side of archaeology that makes her book so very lively. Whether on migration with the Luri tribes in Western Iran or whether dealing with ministry and museum officials or the village locals, she makes very interesting observations on single individuals, and describes them with a lot of humour and at the same time a great deal of sympathy. For example, her depiction of the Iranian inspector on the dig is excellent. He of course is the most crucial figure on any foreign excavation, who is usually torn between a guilty feeling that he should stay on the excavation, perhaps out of fear of his superiors, and on the other hand the great desire to leave the uncomfortable and dirty dighouse and return to his family in Tehran. Black and white photographs of the local Luris and the migrating tribes together with some of the excavation add to the lively text.

Perhaps the only criticism would be whether the general reader for whom this book is designed will be able to follow Miss Goff's story easily. Obscure location names in Luristan – although accompanied by maps – and detailed description of the Iron Age pottery in Western Iran might be rather indigestible for non-archaeologists. But for someone who has worked on archaeological sites in Iran and is familiar with some of the people mentioned, it is a highly amusing and evocative book to read. Nevertheless, one wonders whether all the archaeologists and students portrayed in the book will take kindly to Miss Goff's descriptions of them, which are sometimes surprisingly frank.

VESTA CURTIS

CONNAH, Graham. *Three thousand years in Africa: Man and his environment in the Lake Chad region of Nigeria*. (New Studies in Archaeology). Cambridge, Cambridge University Press, 1981. xx + 268 pp., illus £25.00.

Three thousand years in Africa is a useful introduction to the archaeology of the clay plains (*firki*) south of Lake Chad, and of the Yobe valley to the east of it, explored sporadically by Connah since 1963. Radiocarbon dating evidence from settlement mounds in the former zone spans the period c. 2500–750 BP, and from refuse mounds in the latter spans c. 1000–300 BP.

Three phases of occupation at Daima in the *firki* provide evidence for a series of socio-economic developments from predominantly pastoralist camps to an intensive sorghum-farming community which displays an elaborated material assemblage by 1000 BP. Iron is seen as an enabling factor for such agricultural intensification, the existence of 'Sopots' as grain-storage bins an essential correlate.

The later Kanuri exploitation of the 'Yobe advantage' (a constant water supply in an otherwise relatively arid region) enabled the rise of a Bornu state which eclipsed the emergence of an integrated 'cultural area' further south, in the *firki*, by c. 600 BP.

The balance between stress and opportunity in the human exploitation of 'fragile' environments is promoted by Connah as the unifying theme for what is essentially a study in cultural ecology. Pre-historic resource utilisation is modelled with brief reference to contemporary villages representing the three major 'ecozones' identified, and is to a limited extent tested against the material record from surface collections and eight selective excavations.

The best quality evidence derives from the *firki*, which spread eastwards into Cameroun and Chad where J. P. Lebeuf has unimaginatively defined a 'Sao-culture' from materials collected from the hundreds of mound-sites he has recorded. It remains to be established to what extent this 'culture' corresponds to the Daima III population. In the Nigerian *firki* alone Connah has noted some 36 mound-sites but has excavated only two. The investigation of cultural variability from sites within this

zone is therefore an important next step for archaeologists working in Bornu, in order to qualify and extend the inferences made in this book.

KEITH RAY

NIBBI, Alessandra. *Ancient Egypt and some eastern neighbours*. Park Ridge, NJ, Noyes Press, 1981. xviii + 195 pp., 48 figs., 26 pls. \$24.00.

Following her somewhat controversial work *The Sea Peoples and Egypt* (1975) Miss Nibbi turns her attention to other peoples and regions with which the ancient Egyptians had contact. A key factor in her identifications has been a theory, advanced in her earlier work, that the Egyptians neither engaged in maritime activity during the pharaonic period nor even had in their vocabulary a word for 'sea', both of which unlikely claims have been contested by critics.

In the present volume the author continues in support of her thesis by relocating places previously thought to have involved the Egyptians in sea voyages. Thus in the case of Punt, a region hitherto regarded by most scholars as having been somewhere on the African coast at the southern end of the Red Sea, an attempt is made to place it in Sinai. The fact that one of the products of Punt was giraffes is a difficulty. Although conditions in Sinai at that period might conceivably have supported these animals (the author cites a rock-drawing of one – not in itself proof that they existed there), on balance her proposal is hard to take seriously. Miss Nibbi also suggests the relocation, possibly in the eastern Delta, of the place spelt *Kpny* by the Egyptians, and long thought to have been Byblos. Fuller discussion of this in a later publication is promised.

Regarding the term 'God's-land', which appears to have included Punt, much of the eastern desert, Sinai and southern Palestine, its application was so wide that one doubts if it was really a geographical entity capable of being delimited in the way that Miss Nibbi attempts. Probably nearer the mark is the definition in Gardiner, A. H., *Egyptian Grammar*: 'a generic term for foreign tribute-bearing lands'.

Since the basic premises of the work are dubious, there is little point in detailed discussion of all the circumstantial evidence quoted. One can only regret the expenditure of so much industry and ingenuity. On the credit side the book provides an up-to-date bibliography, some useful environmental data, and particulars of documentary sources. The reader should, however, be warned that the last-named do not always bear the interpretation placed upon them. The fragmentary inscription quoted from Petrie, *Tanis* II, 107, pl. 42, which Miss Nibbi cites as evidence for locating Punt in the north rather than in the south, seems on any reasonable interpretation to imply the opposite.

H. M. STEWART

JAMES, T. G. H. *The British Museum and Ancient Egypt*. British Museum Publications, 1981. 32 pp., 26 figs. £2.50.

A fascinating study of the way in which an apparently random assemblage of curiosities by a private collector in the 18th century led gradually, after its purchase for the nation in 1753, to the establishment of a museum organised on scientific lines and staffed by specialists. The emphasis in the present book is on the Egyptian items, subsequently augmented by travellers, and most notably by the acquisition as spoils of war of a large number of antiquities, including the Rosetta Stone, originally intended by Napoleon for shipment to France. Progress from dilettantism to scientific specialisation was shown by the formation in 1886 of a Department of Egyptian and Assyrian Antiquities, the final separation of Egyptian Antiquities being as recent as 1955. During the time that objects had been acquired mainly through dealers they had been largely unprovenanced. This pattern was broken in the 1880s by the association of the Museum with the work of the Egypt Exploration Fund and eventually by direct sponsorship of excavations, a policy which has continued to the present time.

Perhaps it is slightly unfair to describe the activities of early collectors as dilettantism. As James observes in his admirably perceptive introduction, this was simply the state of science in an age when men 'sought and found interest and wonder in all the phenomena of nature and all the products of human hands from the time of remote antiquity'. Most of the facts related are already known, but it is a pleasure to have them presented in such an urbane and illuminating fashion.

H. M. STEWART

BOOK REVIEWS

Atlat: The Journal of Saudi Arabian Archaeology, Vol. 4, 1400 AH–1980 AD. Riyadh, Department of Antiquities and Museums, Ministry of Education.

Volume 4 of this new journal is largely devoted to an account of recent archaeological exploration in the Saudi kingdom. The first part describes the work done during the fourth season of the Comprehensive Archaeological Survey, concentrated upon the western Nejd and the foothills of Asir. The report highlights the extensive presence in this area of lower and middle Palaeolithic sites, but also recognises the presence of later sites, including house structures and tumuli, probably to be dated between the end of the third and the middle of the first millennium BC. These sites suggest an emphasis on pastoralism rather than on hunting and gathering. A distinctive form of stone tumulus, with a long 'tail' of small stone cairns, is of a type found also in the Yemen and Hadhramaut, and perhaps coincides with ancient caravan routes.

In Part II the continuing survey of the Darb ez-Zubayda, the medieval pilgrim route from Iraq to Mecca, is described. Twenty-three sites were visited, and there is a very full record of plans and photographs. Particularly valuable is the thorough discussion of the water resources, soils, and vegetation of the route.

Part III consists of more detailed studies of two particular sites. At Tayma, in NW Arabia – already known to have been a trading centre of considerable importance, first mentioned in the reign of Tiglath-Pileser III in 736 BC and later to be the domicile of Nabonidus of Babylon in the mid-6th century – a new survey of the town walls and major surviving buildings was carried out. Small scale excavations in one of the buildings revealed a new stele, with long Aramaic text rivalling the importance the famous Tayma Stone found a century ago, and a large stone cube, an altar or offering table, with two faces carved with Mesopotamian religious symbols. The whole complex seems to be of the utmost importance, and would repay excavation.

Finally, a preliminary report on the excavation of a site near Unayza is published. This site, Zubayda-al Amara, is one of several known in the central part of the Kingdom to date to the late first millennium BC to early first millennium AD, and the soundings give us a useful initial glimpse of the domestic architecture and of the Arabian pottery typology of the period, which has connections with Bahrain in one direction and SW Arabia in the other. Further study should reveal more of the pattern of cultural and trading connections in pre-Islamic Arabia.

The volume is excellently produced, with very few typographical errors. For someone not too familiar with Arabian geography, the main criticism will relate to the absence of orientation maps, on which the sites and areas mentioned (especially in the survey reports) can be readily located.

D. B. DOE

SOUTH ASIAN ARCHAEOLOGY 1979 (Papers from the Fifth International Conference of South Asian Archaeologists in Western Europe, held in the Museum für Indische Kunst der Staatlichen Museen Preussischer Kulturbesitz, Berlin). Ed. Herbert Härtel. Berlin, Dietrich Reimer Verlag, 1981. 528 pp., 398 illus., 2 maps. DM210.00. Paperback DM190.00.

This is the latest volume reporting a series of biennial conferences first held in Cambridge in 1971. As with most conference papers there is no common theme and the 36 contributions range from a consideration of the Holocene geomorphology of the Bannu Basin (H. Rendell), through discussions of clay sealings and administrative structures of the fourth and third millennia (E. Fiandra), historical numismatics (A. Simonetta), to the 16th century AD wooden pagodas of Nepal (H. Rau).

Since its inception this conference has grown in authority as well as in size, and its proceedings are now perhaps the single most important venue for the publication of new field research from the Indo-Iranian region and, as such, its publication is eagerly awaited by specialists. With such a wealth of varied material it is clearly impossible to discuss or even to mention separately all the contributions. As in the earlier volumes the geographical emphasis is on the western borderlands of South Asia where teams of Italian, French and, to a lesser extent, British archaeologists have been active over the past two decades, and important new results are presented here from work at Shahr-i-Sokhta in Iran (R. Ciarla, G. Bulgarelli, L. Caloi and B. Compagnoni), Mehrgarh on the Kachi Plain in Pakistan (M. Lechavallier, G. Quivron, J. F. Jarrige and R. Meadow), Shortughai on the Oxus River (H. -P.

BOOK REVIEWS

Francfort), and the Bannu Basin on the Kurram River, Pakistan (F. R. Allchin, H. Rendell, K. D. Thomas, B. Allchin and R. Knox). M. Jansen continues his stimulating re-analysis of Harappan settlement organisation, and L. Constantini documents, through the palaeobotanical remains from Pirak, that in the early second millennium BC the summer crops of rice and millets were added to the traditional winter wheat, barley and chickpea, thus laying the foundation of the modern agricultural system of rural India.

The second half of the volume is devoted to studies of mostly already known materials and although to this reviewer they are less exciting than the primary data coming from new prehistoric field research, there are many important and authoritative contributions. Special mention must be made of J. C. Irwin's detective work on the Asokan (now pre-Asokan) pillar at Allahabad where he shows that many 'established facts' of Indian archaeology are nothing of the sort. Professor van Lohuizen-de Leeuw reopens the old question of where were Buddha images first made, Gandhara or Mathura?, and a young Cambridge student, J. R. McIntosh, provides a historical survey of the megaliths of South India; one of the very few contributions on the archaeology of India proper.

At approximately £50 this is not a book for the interested amateur, and few students, let alone those professionally employed in South Asian studies, will be able to afford it; but the quality and breadth of contributions serves to confirm the series as essential reading for all scholars of Ancient India and its borderlands. As one might expect from such an editor, the book is well structured and presented. It is to Professor Härtel's credit that it appeared just two years after the Berlin Conference.

I. C. GLOVER

RAWSON, Jessica. *Ancient China: art and archaeology*. London, British Museum Publications, 1980. 240 pp., 12 col. pls., 192 figs. £8.95.

This book sets out to be a guide to the Chinese archaeological collections (up to the end of the Han Dynasty, AD 220) in the British Museum, and to set this material in its proper cultural context. In this, Jessica Rawson's book follows a long and distinguished tradition, and it seems to me that she has acquitted herself with great credit, the book being clearly and unpretentiously written, well-produced, attractively laid out, and generously illustrated with plates of almost invariably high quality. There are 12 beautiful colour photographs, not including the dust-cover, which is the inevitable but seductive panorama of the 'Ten-thousand-miles-long-wall' winding across misty ranges of hills, and which is unfortunately not reproduced within the book. Figures 46 and 124 suffer from being over-cropped.

The composition of the book itself reflects the extent of the British Museum's holdings. The neolithic period thus is accorded 13% which seems rather generous (although outrageously stingy if one remembers that it covers at least three times as long a period as any of the others), and 22% is devoted to the Shang Dynasty. The palaeolithic and mesolithic periods are excluded. A little over half of the book is thus devoted to the early historic periods, and it must be pointed out that a historian, which this reviewer is not, might well be more sharply critical. The book is creditably up to date: thus the spectacular and amazing site of Hemudu (published in 1976 and 1978) is included, but the less spectacular but almost equally interesting and still older site of Cishan in Hebei (c. 5,100 bc) published in Wenwu in November 1978 is not.

The museum handbook aspect is also responsible for some features that otherwise seem peculiar in a book with this title and general appearance. There is an index of museum accession numbers (invaluable in front of the display cabinet and worthless elsewhere) cross-referenced to a seven-page catalogue of provenances; but on the other hand not a single C-14 date is cited, let alone tabulated. (The misleading claims of dates in the fifth millennium bc for the Taiwanese neolithic are accepted, although the actual dates indicate the middle of the third millennium.) As is traditional, there is a list of Chinese characters, in alphabetical order of pinyin romanisation, with accompanying Wade-Giles transliterations. Is this worth while? I wouldn't have thought that those who could read the characters would have very much need of such a list, and those who couldn't wouldn't be able to use it in any case. Pinyin romanisation is used throughout the book, and Mrs Rawson gives a useful half page on transliteration and pronunciation, in which however an unfortunate typographical error makes

BOOK REVIEWS

the pinyin 'x' equivalent to 'sh' instead of 'hs', and in which no mention is made of the confusing pinyin 'r', which is pronounced like a French 'j'. The list of Chinese characters uses the traditional forms rather than the revised characters used in current Chinese publications – but as it is easier to convert traditional to revised forms rather than the other way round, this is sensible if inconsistent.

Considering the proper sewn binding, hard covers and wealth of illustrations, £9 is not an unreasonable price for this book. My only major reservation is that it may well be high enough to discourage people from buying it casually for use as a guide. Perhaps a cheaper paper-backed version will be needed?

R. N. L. B. HUBBARD

KIRK, R. L. and THORNE, A. G. (eds.). *The origin of the Australians* (Papers from a symposium at the 1974 Meeting of the Australian Institute of Aboriginal Studies) (Human Biology Series no. 6). Canberra, Australian Institute of Aboriginal Studies, 1976. vi + 499 pp., illus \$12.00.

WRIGHT, R. V. S. (ed.). *Stone tools as cultural markers: change, evolution and complexity* (Papers from a symposium at the 1974 Meeting of the Australian Institute of Aboriginal Studies) (Prehistory and Material Culture Series no. 12). Canberra, Australian Institute of Aboriginal Studies, 1977. ii + 400 pp., illus. \$23.95.

Both of these volumes publish papers presented at symposia of the 1974 AIAS meeting on aspects of both general prehistory and specifically Australian problems. The contents are of variable length, style and quality since some contributions have been published as delivered, some have been extensively revised for publication and some were written after the meetings and present new material. Both volumes are indexed, an unusual but welcome feature. However, although at both symposia the considerable discussion was recorded unfortunately it has not been published. This is a pity since it would have been additionally interesting to know how some of the more contentious contributions were received.

The origin of the Australians comprises 26 papers, of which only that reporting the Lake Mungo material was added after the meeting, divided into five sections. Kirk and Thorne introduce the major problems in Australasian palaeoanthropology: did the initial Aboriginal populations evolve *in situ* from a neandertaloid parent stock; when was Australia first settled; were there later incursions of sapient people from south-east Asia and if so how many and when; why 30,000–10,000 BP does there appear to have been a wide range of skull morphology; what value can be placed on modern genetic and linguistic evidence for the origins and movements of early populations; how does gene flow occur; is there any evidence that racial characteristics, as now defined, were present prior to the evolution of *Homo sapiens*? Possible approaches and answers to some of these fundamental questions are offered in the papers which follow.

Section 1 comprises four papers on aspects of Australasian Quaternary geology. Chappell considers the effect of sea-level on the feasibility of migration. Taking possible isostatic and tectonic movements into account it appears that sea crossings would have been possible since about 53,000 BP, when sea level was at about –150 m, corresponding to the archaeological evidence for human presence in Australia. Calaby's thesis that there is no unequivocal evidence that man, present in south-east Australia since 40,000 BP, played a major role in early Holocene megaherbivore extinction, about 25,000–20,000 BP, is one to which this reviewer is most sympathetic. Hope and Hope suggest that in New Guinea the earliest inhabitants probably preferred the mountain slopes between 1500 and 2500 m, which would have been above the treeline about 40,000–20,000 BP with an open woodland/savannah type of vegetation giving easy access for humans and capable of supporting a high biomass density. The few early sites known from the island support this thesis. Bowler reviews the palaeo-environmental data from the lake Willandra system. There is little evidence for man's presence prior to 40,000 BP when living conditions were arid as now, but from 40,000–15,000 BP the lake system filled due to decreased evaporation not increased precipitation and people camped there frequently; when the system dried out about 15,000 BP they moved elsewhere.

Section 2 comprises four papers on palaeoanthropology, all of which suggest an early Upper

Pleistocene evolved erectid ancestry for Australasian *Homo sapiens*. Jacob reconsiders the Javanese material which he attributes to *Pithecanthropus* because of the lack of evidence for either linguistic or toolmaking abilities. Dating this material is problematic but it probably spans the Lower and early Middle Pleistocene. He sees Australoid origins in the early Upper Pleistocene material from Wadjak, Niah and Tabon. Thorne also thinks this material is ancestral to the Australians, a highly diverse polymorphic population whose extremes of variation are represented by Kow Swamp, with many erectid traits, and the gracile forms from Lake Mungo. MacIntosh and Larnach argue that the Aborigines are descended from an ancient hybrid population which hived off from an early descendant of the Sino-Japanese erectids about 70,000 BP. They represent a single population of surviving archaic *H. s. sapiens*, the extremes of whose diversity are seen in the Cohuna and Keilor crania. Bowler and Thorne describe the Mungo III inhumation, which was probably a gracile male and interred about 30,000 BP.

Section 3 comprises 10 disparate papers on aspects of morphological variation which reach no consensus on aboriginal affiliations. Howells concludes, using multivariate cranial statistics, that modern Asian and Australasian populations, divided into Papuan-Melanesian and Australian groups, are unconnected. While Giles defines four aboriginal groups: Melanesian and Torres Straits: Arnhemland; south-east Australia and Tasmania; Murray River basin, Wallace and Doran conclude that the Tasmanian burials at Mount Cameron West lie within the Australian range of variation. Using physiological criteria from modern Aborigines, MacFarlane concludes that their selective adaptations suggest descent from a tropical ancestor. Abbie argues, from a cross-tribal sample of height, weight and cephalic index, that living Aborigines are polymorphic representatives of an homogeneous gene pool. Using dermatoglyphs, Prokopec and Sedivy argue that the Aborigines are an homogeneous population genetically closest to south-east Asia and Micronesia, while Parsons and White, on similar evidence, argue for heterogeneous mosaic evolution with the Aborigines closest to Melanesia. Wood concludes, on the basis of ossification rates in the modern skeleton, that Papua-New Guineans are closest to Negroids and Caucasoids in their growth rates. Wright suggests that the modern aboriginal palette and teeth are advantageously primitive since small teeth are less likely to decay and relative tooth size appears to be correlated with age at death.

Section 4 comprises five papers on aspects of genetic patterning which present conflicting results and suggest that a definite provable origin for an Australasian Aboriginal parent stock is no longer demonstrable. Lie-Jo, studying the complexities of south-east Asian genetics, concludes that the Aborigines form a separate group, haematologically closest to the Chinese and physically closest to the Indians, while Simmons concludes serologically that there is no obvious Asian parent stock for the Aborigines, who form a group apart. Kirk, also using serology, concludes that the Aborigines' closest link, a tenuous one, is with the Malaysian Mongoloids. Curtain *et al.* conclude from immunological studies that the highly heterogeneous Aboriginal gene pool is closest to that of Papua-New Guinea, while Omoto and Misawa disprove the hypothesis that the Ainu and Aborigines are sibling populations derived from a common Upper Palaeolithic Asian ancestor. The Ainu are aboriginal Japanese.

Section 5 comprises three papers on evolutionary genetics, which also fail to provide a single easy answer to Aboriginal origins. Mutton and Keats, studying kinship systems, suggest that the Aborigines are most similar to the south-east Asian Mongoloids but that the very diverse populations suggest either a great time depth since colonisation or a small founder group. Sanghri concludes that serological allele variation suggests little connection between living Indian and Australian populations. Finally Booth and Taylor suggest that if calculations of genetic distance between New Guinean populations and linguistic classifications are correlable then the construction of lexic time scales is possible. They would indicate degrees of chronogeographic separation amongst widely dispersed populations, for example that the Torres Strait became an effective barrier about 10,000 BP. While all these papers present much helpful new information, it is clear that there are still many problems outstanding in the search for Australian origins.

Stone tools as cultural markers comprises 32 papers, printed in alphabetical author order; this unfortunately obscures any underlying themes within the overall topic. The first paper, by Glynn Isaac, is a specially written overview in which he tackles the problem of the fundamental reliability of stone tools as cultural markers. In Europe, where stone tools show a greater variety of morphological patterning than elsewhere, they have perhaps been liable to over-interpretation. For example, most European typological systems assume an interconnection between form and function. In the

Australian living Stone Age, where the simplicity and heterogeneity of most artefacts makes their interpretation difficult, analysis has shown that form and function are not necessarily correlable. Isaac, therefore, questions the usefulness of changes in stone tools morphology alone as a cultural indicator, emphasising the effects of ecological, economic or activity differences which are more difficult to assess. He also questions the uncritical use of attribute analysis to determine cultural differences, emphasising that attributes must be carefully selected for their relevance to the questions to be answered.

The papers which follow group into several disparate topics. Allbrook and Freedman discuss hominid forelimb functional morphology especially halux rotation which permitted development of the precision grip considered essential for delicate tool manufacture and use and probably led to enhanced biocultural evolution as cerebral organisation increased in complexity. Pongids lack this manual adaptation and, therefore, the thumb is negligible in tool use. However, they fail to resolve the continuing debate on the tool-making abilities of *Australopithecus*. Barrett discusses ethnographic evidence for the use of anterior teeth for pressure flaking stone and the detection and analysis of resultant incisive wear patterns in skeletal remains.

Some problems in Old World archaeology are considered. The late Professor Bordes' admirably brief summary of his last thoughts on the 'Mousterian question' casts doubt on the validity of the Typical variant while the floruit for the whole Mousterian is extended to the late Riss, about 150,000 BP and modern man is included as a manufacturer. Ghosh considers the *Homo erectus* material from Java and argues for a general early south-east Asian pebble tool tradition and suggests Australian parallels for the later Wadjak and Solo hominid material. Shackley's paper on the chronological and cultural implications of British and north French Middle Palaeolithic bout coupé bifaces is marred by the misuse of geological terms, e.g. Monastirian and Wurm I, which have no place in the local stratigraphy. Finally Walker discusses the Terminal Palaeolithic-Mesolithic transition in Spain as demonstrated at Abrigo Grande, where about 7,000–5,500 BP initial ceramic technology was added to an unchanged lithic tradition. He, therefore, argues that 'Neolithisation' was not intrusive here but an internal development.

Binford's cautionary study of hunting behaviour among the Nanamuit Eskimo demonstrates that the more important a tool is to its maker the less likely it is to be found in the archaeological record!

The remaining papers cover aspects of Australasian stone technology. Coutts, Hayden and Kaminga consider microwear analysis showing that the type and degree of edge damage on a tool is related to its raw material as well as its function. Hayden considers the function of Mousterian factor IV tools, described by the Binfords, as food processors unlikely. Several recent studies have demonstrated that the majority of functionally identifiable Palaeolithic tools were used for wood working. While this may well be correct, it could be that many tools were multifunctional of which only wood-working traces survive, since the damage caused is particularly heavy. Kaminga's study shows eloueras to have been hafted for wood working also.

Crosby's thought-provoking paper attempts an archaeological classification of ethnographic material. Using Boolean numerical taxonomy she tested the resultant types against morphology instead of defining them by it, as is more common. They cross-cut known linguistic boundaries and she suggests that the basic assumption that morphology, style and social group are co-terminous is incorrect.

Several papers consider archaeological evidence from specific areas. Dortsch surveys the early prehistory of the Kimberley region. Golson considers the lack of impact of the introduction of agriculture on an unchanged lithic tradition in Timor and New Guinea. Jones and Lourandos discuss the simplicity of Tasmanian technology and the cultural effects of post-glacial isolation due to sea level rise. MacBryde demonstrates cultural differences between coastal and tableland groups in New England. Pretty contributes a lengthy report on the Roonka Flats burials, while Lampert's report on the Seton site also discusses the abandonment, about 11,000 BP, of Kangaroo Island as rising sea level reduced its viability. Gould and O'Connell discuss central Australian Aboriginal technology, where stylistic continuity and quality of raw material are crucial. Finally, Pearce, Stockton, Vanderwal and White *et al.* discuss aspects of typology, the latter's discussion of the distinction between and identification of cognitive types among Duna stone knappers is particularly useful.

Together these two volumes provide a wide selection of recent discussion on matters Australasian which are more germane to Eurasian prehistory than might at first be apparent.

ESMÉE WEBB

BOOK REVIEWS

LYNCH, T. F. *Guitarrero Cave: early man in the Andes* (Studies in Archaeology). New York and London, Academic Press, 1980. xx + 328 pp., illus. £13.00.

Guitarrero Cave is situated in the Callejón de Huaylas, a high intermontane valley in the Andes of northern Peru. As the crow flies, the Callejón is less than 100 km distant from the Pacific coast, with at least two relatively easy access routes.

The primary aim of the interdisciplinary investigation headed by Lynch was to examine the possible exploitation – by migrating populations in preceramic times – of a variety of seasonally available resources ‘through the steep gradient of Andean altitudinal zones’ (p. 5), perhaps extending as far as the coastal desert. (The word ‘transhumance’, used by Lynch in this context, is incorrect when applied to movements of people; it is essentially a pastoral term, meaning ‘the seasonal moving of live-stock to regions of different climate’ (*Shorter Oxford English Dictionary*, 2nd edn).

The cave was promising owing to its size, its location near the flood plain of the valley and the thickness and dryness of its deposits. In practice it presented some serious difficulties. Not only were the deposits much disturbed by intrusive burials and the subsequent activity of looters, but the front of the cave is separated from the back by an outcrop of bedrock. As a result evidence from the excavation units in the four stratigraphic complexes varies greatly in reliability. Both Lynch and the scientists engaged in the project are scrupulously cautious when assessing individual finds, but they can get carried away in their overall conclusions. Patterson, in his review in *American Antiquity* (July 1981, 46(3)) points out the problems in the carbon dating arising from the stratigraphy.

Another difficulty was the scarcity of comparative material from other preceramic highland sites. In the case of the pollen analysis for instance, R. Kautz and B. Hatoff had no available pollen information from the central Andean area and unfortunately no off-site control samples were collected either. Many specimens amongst the exceptionally large quantity of plant remains had never been found archaeologically before, so that an exhaustive collection of modern local flora had to precede the study of the remains. Even so, underground plant parts (roots, tubers, rhizomes) could not be identified as wild or cultivated, due to lack of comparative material. The same applies, though to a lesser degree, to the faunal remains: the *Camelidae* (guanaco, vicuna, llama, alpaca), for instance, could only be identified to the familial level, making the assessment of early domestication impossible.

The general impression emerging from the study of the remains is one of strong climatic, environmental and cultural continuity. The cave was probably in permanent use as a seasonal base-camp from the eleventh/tenth to the sixth millennium BC and then sporadically or for burial purposes till about AD 550. Most elements of today’s local diet (beans, tubers, maize, peppers, squashes) are already present in the preceramic strata. The beans from Complex II (no plant remains survive in Complex I) are the earliest known in the central Andean area and prove that beans were already under cultivation by 8000 BC. However, as no wild beans are found in this area one cannot assume that their domestication started here.

As for cultural remains – especially the chipped stone assemblages – Lynch attempts to fit them into a wider South American context, trying to show a general progression from unifacial to bifacial industries, as well as strong similarities with other Peruvian highland sites like Lauricocha and Pachamachay. He is at pains to point out the difficulties of drawing conclusions from ‘archaeological data of uneven quality’ and prefers to work only with the ‘best’ albeit small ‘data’. But problems may also arise from Lynch’s extremely exhaustive typology, based on relatively small samples from two other sites in the area (Quishqui Puncu and Lampas). Perhaps no large-scale comparisons should be attempted, until a more broadly based and less minutely differentiated typology can be worked out from larger samples and until more sites have yielded unequivocal stratigraphic sequences.

The textiles and cordage from Complexes II and III, studied by J. M. Adavasio and R. Maslowski are, if correctly dated, the earliest in South America. The fibres used are not cotton.

From the total absence of ‘imported’ remains in Guitarrero Cave, Lynch concludes that his hypothesis of wide-ranging seasonal migrations has been disproved. But the existence of different types of sites within the Callejón (small rock shelters, large open camp sites at altitudes too high for year-round occupation) leads Lynch to propose a model of locally limited ‘transhumance’, as an adaptation of a growing population to depleted resources, ‘a sort of palliative precursor to intensive sedentary agriculture’ (p. 312). This model differs, for instance, from Rick’s model for the Pampa de Junin, where the high plateau environment with little climatic seasonality offers the same range of resources

BOOK REVIEWS

throughout. It resembles more the situation found by MacNeish in the Ayacucho Basin, although that area is much more open than the Callejón, but lower and more varied in its micro-environments than the high puna. On the whole, Lynch's model is plausible for a valley like the Callejón, where rapid variations in altitude mean that short-range movement gives access to very different resources. His study fills a useful gap in our expanding picture of early man in the Andes.

A. MORGAN

MEIGHAN, C. W. (ed.). *The Archaeology of Amapa, Nayarit* (Monumenta Archaeologica 2). Los Angeles, University of California, Institute of Archaeology, 1976. 306 pp., 200 pls. \$35.00.

The excavations reported in this monograph were carried out in 1959, well before the recent growth of the theory-building industry. The publication, in spite of its 1976 date, inevitably reflects the aims and interests of earlier days: a respect for basic, descriptive data, a preoccupation with questions of typology, sequence and chronology, and a belief that what the archaeologist should be doing is writing the history of his chosen area. Given the state of West Mexican archaeology in the late 1950s, with a very few excavated sites scattered over a vast area, these priorities were the right ones at the time. As the Editor writes in his Introduction, 'The collection from Amapa is an important one and still the largest controlled excavation along the West Mexican coast; it provides, therefore, not only a good chronology of the culture change in this part of Mexico but a large and varied collection of great comparative value in understanding some of the smaller test excavations and surface collections that have been made'. This remains true today.

Amapa was a complex of earthen mounds, cemeteries and rubbish heaps spanning the period c. 300 BC to AD 1400. Excavation conditions were difficult. Many of the mounds were built up from redeposited fill (of little use for dating), and most of the burials were below the water table. By a judicious mixture of stratigraphy and seriation, Meighan's team constructed a sequence of five (or possibly six) phases, which were then dated by obsidian hydration and by cross-links with other sites. The three radiocarbon dates were unacceptable. This general sequence has stood the test of time, and the absolute chronology has been revised to take account of new discoveries up to the date of publication.

Like the other volumes in this series, standards of publication are sumptuous, with good paper, wide margins, and 281 pages of photographs (whose pagination does not always correspond with the List of Illustrations). Meighan himself describes the site, its stratigraphy, architecture, dating, and material other than pottery. There are appendices on the fauna and the human skeletons, and, tucked away among these minor contributions, an article by Lawrence Feldman on 'Greater Nayarit Archaeological Molluscs and Archaeo-molluscan Units in Northern Mesoamerica'. With its discussion of the cultural implications of shell collections, this is an important paper in its own right. The final sections are a detailed description of the Ball Court excavation by Francis Clune, and a long chapter on the Amapa pottery by Gordon Grosscup. This is an updated version of Grosscup's doctoral thesis, and should be read with Betty Bell's companion dissertation on the whole vessels from the tombs. Although Bell's text is not available, the funerary vessels are illustrated. Grosscup is unusually frank about his difficulties — plain sherds dumped to save shipping costs, deposits mixed and disturbed, bags mislabelled, etc. — and he offers a good practical man's discussion of the problems of employing the type variety system of classification to complete vessels rather than to sherds. In his experience, as in mine, the two groups of material are often impossible to match up, and may give quite different figures. As Meighan points out (p. 141) fragments of a single elaborate vessel type may be classified into as many as 10 or 12 sherd types. Having made his complaint, Grosscup does not draw the obvious conclusion that perhaps an analysis in terms of modes, rather than of types, would have been more appropriate for the Amapa wares, whether sherds or whole pots.

All in all, this report is a good piece of descriptive Old Archaeology, of the kind that will keep its value while theories and methodologies come and go. As a record of an important excavation, it will remain a basic source-book for everyone interested in West Mexican archaeology.

WARWICK BRAY

ASHMORE, Wendy (ed.). *Lowland Maya settlement patterns* (School of American Research Advanced Seminar Series). Albuquerque, University of New Mexico Press, 1981. xviii + 465 pp., illus. \$30.00.

This is the third of three volumes of the School of American Research Advanced Seminar Series on the archaeology of the lowland Maya. The previous two, *The classic Maya collapse* and *The origins of Maya civilization*, were, in my opinion, competently produced and provided good accounts of the state of our knowledge on the Maya collapse and the Maya origins, respectively. Participants of the first two seminars agreed that many of the assumptions and interpretations of Maya society depended upon information about Maya settlement patterns, e.g. land use, population estimates, social, political and economic structures. Hence, because of its vital role in the reconstruction of ancient Maya society, a seminar on lowland Maya settlement patterns was held and this text is the result.

The text is divided into four parts. Part 1 consists of five chapters concerned with frameworks for studying lowland Maya settlement patterns. The first chapter (Ashmore and Willey) provides a brief historical outline of the study of Maya settlements. The second (Hammond and Ashmore) places the Maya in geographical and chronological context, with an emphasis placed on the heterogeneous nature of Maya lowlands geography. Chapter three (Ashmore) is a thorough account of some methodological and theoretical issues in lowland Maya settlement patterns studies. Among other things, Ashmore points out the assumptions and definitions that our interpretations, particularly related to function, are based upon. The fourth chapter is an innovative one. Turner, Turner and Adams present a new method in assessing hierarchical ranking of Maya centres. A method of ranking Maya sites is vitally required in order to make some sense of the distribution of Maya settlements. This trio have done so by assessing them on the basis of physical dimensions. And finally, the last chapter of the section (Haviland), is an investigation of an architectural complex within Greater Tikal. With the aid of epigraphic data Haviland reveals that it was likely to have been an élite residence. I am pleased to see that epigraphy is being used more frequently as an aid in interpreting archaeological data.

Part 2 consists of seven chapters providing a regional synthesis of Maya settlements. The sixth chapter (Rice and Puleston) concentrates on the Peten, seventh (Hammond) on Belize, eighth (Leventhal) on the Copan and Motagua valleys of the south-east, ninth (Adams) on central Yucatan and southern Campeche, tenth (Harrison) on southern Quintana Roo, eleventh (Kurjack and Garza T.) on the northern Maya area of Yucatan and the twelfth (Freidel) on continuity and disjunction of post-classic settlement patterns in northern Yucatan. Each author concentrates on a particular aspect of settlement in each of their respective regions of study, and consequently the chapters in this section are not simply descriptive reviews of regional patterns of settlement. Some important observations and interpretations are presented.

Part 3 consists of three chapters on processual models for Maya settlement. Each author uses models derived from non-Maya ethnographic and historical analogy in order to integrate patterns of settlement with other aspects of Maya society. Adams and Smith (chapter 13) make a cross-cultural comparison between the social and political structure of the feudal societies of medieval Europe and Japan with the Classic Maya on the basis of similarities in dispersion of their respective forms of settlement. In chapter 14, Sanders attempts to correlate Maya settlement distribution with infield-outfield agricultural practices. In areas of dispersed settlement outfield practices of terrace and raised field agriculture generally occur and in areas of nucleated settlement the infield garden plots occur. Freidel (chapter 15) produces a model to explain the method of distribution of products in Maya society. This model of distribution, the pilgrimage-fair model, combines the migration of people under religious sanction (pilgrimage) with an exchange of goods and services at fairs associated with these pilgrimages. Each of these models concentrate on different aspects of society but the three are certainly complementary and may come close to the actual Maya situation, however speculative their arguments may be. But I cannot for a moment think how Freidel's argument could be tested archaeologically and because his model does not have any specific historic or ethnographic source it is definitely the weakest and most speculative model of the three. It is interesting but not as convincing as the other arguments presented by Sanders and Adams and Smith.

Part 4 consists of one chapter – Gordon Willey's summary review of the text. Willey was the first to use the term settlement pattern in archaeology within his Viru valley project in Peru and it was he who brought the term and study to Maya archaeology in his Belize valley (Barton Ramie)

BOOK REVIEWS

settlement project. There could be no one better to summarise a text of this sort and he does not let us down. Willey points out the difficulty in establishing a central problematic theme in Maya settlement pattern studies because of the diversity and specialisation of aims and strategies. Aims have focused upon population estimates, subsistence adaptations, micropattern details of architectural layouts in individual centres, and macrosettlement problems of site sizes and hierarchies. This diversity is evident in the text as respective authors emphasise each of the particular problems with which they are concerned. But by Willey's guidance the material is synthesised together again in a common perspective. In doing so he indicates the problems, difficulties, advantages and disadvantages of the various viewpoints and approaches of the contributors to the text. The summary is worth the price of the text alone.

Lest I give the impression the text is perfect, I must make a criticism regarding one particular chapter. In the fourth chapter, Turner, Turner and Adams present a new method in establishing hierarchical ranking of Maya centres. The method is rather complex but it essentially involves the quantifying of the architectural and spatial elements of centres with the cultural elements to produce a volumetric measure. This is then converted to a linear value to be used as a value for the hierarchical ranking of a centre. Superficially it appears to be a brilliantly organised statistical operation for ranking sites, assuming we would have enough data from a centre to be evaluated. And it appears to be a significant advance to the simple methods of assessing rank by counting plazas (Adams) or stelae (Morley). My criticism rests with the lack of justification for specific procedures and operations. In particular, there is no justification whatever for the segmenting of the height of structures into various levels according to the Fibonacci series. Why is it done and how much does it affect the statistical operation? This is important because the authors very correctly indicate the existence of regional architectural styles. Does the segmenting of structures according to the Fibonacci series neutralise this difference? I, for one, wish to find out. In addition, I am still uncertain as to the precise details of the weighting of a value for the cultural elements of a centre. There simply is not enough description and illustration of the details of the operation. It would have been very useful for the authors to have carried one example right through the entire statistical operation in addition to outlining and summarising the approach. This may not be the place for such presentation. The authors, however, mention that a monograph is in preparation on the development, description and application of the system and it would have been appreciated had more of these comments been included here. In the end, one is left with the impression that they are really on to something, but the reader cannot quite sort it all out. I hope the forthcoming monograph will fill in the gaps. I also hope that this physical assessment of the hierarchical rank of centres may, in future, be integrated with rank assessment based on hieroglyphic evidence (emblem glyphs). But that is for the future. The Turners and Adams are at least bringing us one step forward.

In conclusion, this text has the quality of composition and quantity of information to become an indispensable standard reference on Maya settlement patterns, and it has arrived just when it was needed. What more can one demand?

W. B. M. WELSH

CLARK, Grahame. *Mesolithic prelude – the palaeolithic–neolithic transition in Old World prehistory*. Edinburgh, University Press. viii + 122 pp., illus. £4.50.

As a synthesiser of world events in prehistory, no one is more accomplished than Grahame Clark. This latest slim but densely textured volume once again displays his prodigious talent in this field. Like its pioneering predecessor of 30 years ago, the book has been developed from a Robert Munro lecture given in Edinburgh and deals with the continuity of history – in particular the continuity between the hunter-gathers of the Old Stone Age and the farmers of the New.

An introduction briefly outlines the history of Mesolithic studies: the hiatus hypothesis, Childe's Neolithic Revolution, Morgan, Marr, Mongait, and – more recently – Mathyushin, whose views are closer to Clark's own. The first section of the book then deals with south-west Asia. However, before discussing the data from this area, already well known to students of the period, the author makes excellent use of the evidence now available from the New World to help us examine this data with new insight. In Mexico and Peru, in particular, the work of the Archaic period has, since the

1960s, revealed an increasingly detailed sequence of events in the evolution of food production – based chiefly on plants. Moreover it is interesting to learn that pottery not only appears late in this sequence but ‘at least five thousand years after undoubted evidence for . . . domestication’ thereby following a similar pattern to that already known from the Old World.

There follows a resumé of work by Robert Briadwood and his colleagues in north-east Iraq; by Hole and Flannery in south-west Iran; and by Garrod, Kenyon and many others in Palestine and Jordan. In all these regions a broadly similar pattern emerges: the ‘hiatus’ or ‘gap’ (our Mesolithic) between the hunting-gathering economies of the late Pleistocene and the farming economies of the Neothermal is seen to be illusory. Indeed it is the ‘Mesolithic’ populations who are shown to have achieved this transition from hunting and foraging to food production, thereby ‘laying the economic basis for the rise of the earliest cities . . .’ and all that that implies.

The third chapter deals with Europe, the author’s special area of study, and is a masterly summary of the ecology, technology and man/animal relationships during the period, including the importance of fishing and coastal resources. Clark points out that, in contrast to the part played by animals, the economic role of plants in the European Mesolithic is less well known, and the evidence is still scanty. The remarkable and rather rapid spread of blade and trapeze industries over much of continental Europe in the later Mesolithic is contrasted with their sporadic diffusion in the Mediterranean and Atlantic regions, suggesting that the latter was at least partly seaborne.

The last chapter, entitled ‘The Neolithisation of Mediterranean and Temperate Europe’, summarises the evidence for the earliest domestication of plants and animals in the key regions: Greece, south-east and central Europe, the Alpine foreland, north Europe, the Mediterranean basin and the Atlantic seaboard. It includes an interesting digression on megalithic tombs, with their distribution/diffusion linked to the quest for fish as an important factor in extending the sea routes. (The author is surely right to stress the role of fish in Europe in prehistoric times, especially shoal fish such as tunny and cod. In this connection one might speculate whether seasonal migrants such as the salmon might explain some of the later prehistoric settlement in the highland valleys).

To those of us who have, since the 1950s, followed Clark in seeing the Mesolithic as a dynamic period, and indeed ‘an essential prelude to fundamental advances in the development of culture’, this book comes as no surprise; it is a concise and welcome testament to those beliefs. For those to whom the period has seemed to offer little of interest – full of ‘dreary microliths’ and little else – this small book may serve to open fresh windows on an important stage of mankind’s development and demonstrate the value of the ecological approach, so long espoused by Clark and now generally accepted. For, as Clark points out, the earlier communities which gave rise to advanced civilisations can only be fully understood by studying the (often lengthy and complex) process of transformation of their economic systems from hunting/foraging to farming. Moreover, there are still important areas (India, China and elsewhere) where this transitional stage – Archaic/Intermediate/Mesolithic or whatever – is still poorly known, resulting in ‘major gaps in our knowledge of world prehistory’.

Mesolithic prelude presupposes some background knowledge of the period and is clearly aimed at the serious student. Useful summaries of the archaeology of the key regions make it an excellent introduction for extra-mural students and for those in related disciplines. It should be obligatory reading for all students of archaeology whose courses begin with the later prehistoric periods.

As is to be expected from this publisher, the book is well produced with good quality illustrations, a useful bibliography and an index. The reasonable price puts it within reach of most readers.

PATRICIA M. CHRISTIE

CHAPMAN, R., KINNES, I. and RANDSBORG, K. *The archaeology of death* (New Directions in Archaeology). Cambridge, Cambridge University Press, 1981. 159 pp., illus. £17.50.

This book is the second of a series which aims to combine theory and methodology with factual evidence, avowedly a product of the New Archaeology. Although designed to have a unified approach, it is in fact based upon the Prehistoric Society 1979 Spring Conference of the same title, though only some of the papers were written for that occasion. There are 10 papers, four English, one Danish and five American; all deal with prehistory except Klaus Randsborg’s essay on Viking age Denmark. Nearly all the authors acknowledge their debt to a book which appeared a decade ago, *Approaches to the*

BOOK REVIEWS

social dimensions of mortuary practices, edited by James Brown, which contained seminal papers by Binford, Browne and Saxe; all those who used that volume will find this a vital follow-up and excellent reappraisal of current research.

The initial chapter, by Robert Chapman and Klaus Randsborg, is perhaps the most valuable, being an historical and bibliographic survey of the subject offering a useful summary of the approaches and techniques employed, especially in recent years. Three chapters of theoretical discussion linked with case studies follow; James Brown reviews the possible demonstration of ranking in a society's mortuary data, using arguments based on analysis of effort and expenditure, or the distribution of 'symbols of authority' and on the age/sex distribution. In a similar way, John O'Shea's case study identifies elements of social ranking but not kin-based social subdivisions; Lynne Goldstein emphasises the importance of spatial analysis in all aspects of mortuary studies (surely this is rather obvious nowadays?), with key variables of body-treatment, preparation of 'disposal-facility', burial context and demographic/biological dimensions being used in the search for social organisation. Three British papers deal respectively with a reappraisal of the transition from the Mesolithic to the Neolithic, dealing with mortuary data in anthropological terms (Robert Chapman), Neolithic chambered tombs (Ian Kinnes) and another look at 'urnfield' burial, dealing with the relationship between burials and other land use and settlement in southern Britain (Richard Bradley). Klaus Randsborg contributes a useful complement to his 1980 book *The Viking age in Denmark* but breaks no new ground. Lastly there are two more specialised paleodemographic case studies, by Jane Buikstra on subsistence adaptations at Koster (but she only has 25 skeletons spread over 3,000 years . . .) and by Della Cook using age-structured stress indications in teeth to demonstrate status.

The scope of this book is not as wide as the title would lead us to believe, with the emphasis primarily on social aspects of prehistoric mortuary practice in Europe and America. There is little reference to historic archaeology and the palaeodemographic chapters need expanding to be representative. It is a pity that what is much more a conference proceedings, with stimulating discussion of current research, than a textbook should appear in hardback with a grandiose title. A pity also that the American contributors write totally opaque prose and make identical bibliographic references. But the book is enjoyable, especially Chapman's contributions, stimulating and does indeed 'explore', as promised, *some* of the 'frontiers and potential of research'.

SEBASTIAN P. Q. RAHTZ

SHEPHERD, R. *Prehistoric mining and allied industries* (Studies in Archaeological Science). London, Academic Press, 1980. xii + 272 pp., 102 illus. £16.00.

This survey of prehistoric mining will be welcomed by everyone interested in ancient metallurgy as the author surveys the extraction and processing techniques used not only for copper, tin, iron and the precious metals but also for flint, siliceous, obsidian and other hard stones. It is unfortunate that chlorite is not included in view of the considerable amount of recently published work on problems of manufacture, and distribution of objects of this material in Iran and Western Asia. Lapis lazuli receives only two lines without mention of the survey of the lapis mines of Badakshan in north-west Afghanistan undertaken by Georgina Hermann in 1964 and important sites such as Tepe Yahya and Shahr-i-Sokhta are not mentioned.

Archaeologists will certainly agree with the author about the need for the involvement of mining engineers in the discovery, excavation and interpretation of ancient mining sites and in Chapter 6, 'Chronology and Customs Associated with Siliceous and Stone Mining' it is good to find ten questions relative to the activities of the actual miners clearly listed which are often ignored by archaeologists.

There is a useful summary of the mines at Rudna Blava in Yugoslavia and Aibunar in Bulgaria (illustrated by photographs with no scale), but while Kozlu in Turkey, Veshnoveh in Iran, and Timma also are discussed, it is clear that the book is aimed primarily at students of European prehistory. J. B. Richardson's *Metal Mining* (1974) should therefore be added to the references.

However, a section of the Near East in the Copper and Bronze Age mining chapter is included although confined to one and a quarter pages, with a confusing lack of references to original published sources, so that important sites such as Cayonu are not referred to in the publications of the excavator. Again, while the occurrence of tin in Cornwall is noted and the tin bracelet from Lesbos

BOOK REVIEWS

mentioned, speculations on the source of Lesbos tin are misleading without a brief account of recent work on ancient tin which could assist students in following up references such as those conveniently given in the published report of the 1977 Smithsonian Institution Seminar – *The search for ancient tin*.

The section on the history of the use of iron can now be read in conjunction with *The coming of the age of iron* (reviewed below) and so the lack of references here is not so confusing. But dates must be used with caution; the iron found by Woolley at Ur came from a grave of the Early Dynastic III period c. 2600–2350 not 3000 BC as stated by Mr Shepherd; the sources of iron used by the Assyrians have been discussed by several scholars since 1948 (Pleiner and Björkman, 'The Assyrian Iron Age', *Proceedings of the American Philosophical Society* 118, 1974) and the reviewer in *Iraq* (1974), 'Assyrian Sources of Iron'.

In the section on gold the statement that the first gold was probably obtained from stream beds as early as the sixth millennium BC is not followed up with any mention of early occurrences such as the fifth and fourth millennia BC examples of gold found at Ur and Gawra in Al Ubaid and Late Uruk contexts, or the goldwork from Varna.

Finally, one hopes that the author will in the future expand the useful chapters on the elements of early mining and bring the references up to date.

K. R. MAXWELL-HYSLOP

WERTIME, T. A. and MUHLY, J. D. (eds.). *The coming of the age of iron*. New Haven and London, Yale University Press, 1980. xix + 555 pp., illus. £14.20.

The appearance of *The coming of the age of iron* is an event of far-reaching importance for scholars in many different disciplines for whom the problem of the discovery and use of iron in areas such as Europe or western Asia is here set in its wider continental setting. In 14 chapters, whose geographical dimensions range from south-east Asia to Africa and the Andes, the beginning and development of iron technology is treated by archaeologists, metallurgists and anthropologists, each expert in their own field. We are thus presented with excellent summaries of the extent of modern knowledge with detailed footnotes and references to previous work in each specialised field, combined with discussion of the many complicated and still unsolved problems which confront scholars today. Certainly no more worthy recipient of such an admirable synthesis could be found than Cyril Stanley Smith, Professor Emeritus at the Massachusetts Institute of Technology. No doubt it was his breadth of vision that inspired the editors of this volume to cover a period from Mousterian (Denise Schmand-Besserat's account of 'Ochre in Prehistory; 300,000 Years of the Use of Iron Ores as Pigments') to the activities of blacksmiths in Ghana in 1972 and extending from the Andes (Heather Lechtman, 'Metallurgy without Iron'), across Europe (Radomir Pleiner), the Mediterranean (Anthony Snodgrass), the Near East (Jane Waldbaum), Iran (Vincent Pigott), East and south-east Asia (Joseph Needham) and Africa (N. J. van der Merwe). In addition, the Bronze Age background to this achievement is admirably treated by one of the editors, James Muhly, and 'An alternative sequence for the development of metallurgy: Tepe Yahya, Iran' is put forward by Dennis Heskell and Carl Lamberg-Karlovsky. Moreover, the *sine qua non* for the intelligent understanding of the historical, archaeological and ethnographical conclusions concerning the development of all aspects of early ironworking is given by the contributions of Charles, Tylecote, Pleiner, Stech-Wheeler and Maddin, and it is these chapters combined with the pyrotechnological background by T. Wertime which emphasise the enormous advance made since seminal studies made as early as 1941 by Coghlan and 1944 by Childe. And as the excellent introduction which summarises the conclusions advanced in the different chapters states, 'This book does not select among these theories or try to rectify differences of outlook which have moved various authors'.

Lamberg-Karlovsky disputes the assumptions that 'technological innovations lead to rapid and major changes in social institutions', and concludes with the view that 'it is the social milieu . . . that is the background of the causes of change'. This is based on the evidence from one site, Tepe Yahya which is confined to the fourth millennium BC and while few would dissent from his statement that 'evolutionary schemes of metallurgical development . . . fail to deal with the problem – solution of the effect of technology on ancient societies', it is surely questionable whether a theory

based on one site in Iran in the fourth millennium can be applied to societies such as the Hittites in the second or the Assyrians in the first millennia, where textual evidence can supplement the archaeological record of the economic and political activities of the inhabitants, merchants and rulers in these areas.

Furthermore, Muhly emphasises the importance of old Assyrian texts because they 'give us our first glimpse into what we might call the economics of the ancient metal industry' . . . and also shows how a study of the perennial problem of tin in the ancient world cannot be isolated from evidence for trade in copper and tin yielded by the Mari texts. Problems concerning the translation of terms for metal such as *aš'iu* and *amūtum* discussed by the writer of this review as far ago as 1972 are here reconsidered by Muhly who is inclined to accept the translation of *amūtum* as bloom iron. Furthermore, Wertime suggests that *aš'iu* might denote the black sands of the Black Sea coast, and the article by Tylecote in *Anatolian Studies* throws more light on this intriguing problem. In addition, the activities of the merchant Ea-Našir at Ur, whose house was excavated by Woolley in 1931–2 are also discussed by Muhly and remind the reader that some of the individuals who actually bought, sold, manufactured and used metal artefacts, can be identified and studied along with a mass of economic textual evidence relevant for those primarily interested in the 'socio-economic status of the craftsmen'.

This collection is, therefore, a splendid example of the vital necessity of co-operation between archaeologists, metallurgists, technologists, linguists and historians if a true picture is to be made of the development of the idea of using iron as a material for tools, weapons and ornament. A specialised approach, which is unaware of the latest work in each of these related fields can only result in an inadequate or even false interpretation of the evidence. Of course no one is better qualified than the recipient of this volume to evaluate and draw conclusions from the mass of evidence which it contains. Therefore, it is perhaps surprising that the one aspect which one can suggest is missing in this symposium is one which Cyril Smith himself elaborated in his Penrose Memorial lecture in 1971 and also in the *New York Times* with the provocative title 'Aesthetic Curiosity – the Root of Invention'. (See also his Preface to *Early Technologies* edited D. Schmandt-Besserat.) Here the view is advanced which can be briefly summarised that 'Necessity is not the mother of invention – only of improvement. A man desperately in search of a weapon or food is in no mood for discovery; he can only exploit what is already known to exist. Innovation and discovery require aesthetically motivated curiosity; they do not arise under the pressure of need.' In developing this idea Cyril Smith demonstrates that 'Most of man's inventions have first appeared in decorative rather than practical applications', and that 'the making of ornaments in copper and iron certainly preceded their use in weaponry'. Among the many examples used to illustrate this viewpoint the fine repoussé plaques from Hasanlu are cited. Publication of these plaques is badly needed and they are rather surprisingly omitted from any mention in this volume. But perhaps the description of the smelted iron sands by Theophrastus (quoted by Wertime) as 'being iron far more beautiful than the other kinds; for if it were not burned in the furnace it would not differ at all, it appears, from silver' . . . and 'not liable to rust' might provide another instance of aesthetic curiosity. The penetrating account of the gold-dominated metallurgy of the Chavin culture in the Central Andes given by Lechtman again is relevant to Smith's theory, and it is especially in this chapter that due attention is given to the questions of not only *how* but *why* metallurgy developed as it did in any given setting and at any given time. Lechtman also stresses the necessity of comparative anthropological data when comparing the evidence for early metallurgy provided by archaeological sites all over the world.

The *embarras de richesses* offered here for thought and discussion is presented in a way that precludes detailed criticism and it only remains to mention a few omissions. Tylecote's suggestion that meteoric iron could have been mixed with smithed material (*History of Metallurgy*, 42) is relevant for any discussion of isolated early examples such as the axe from Ugarit and in connection with references to Piaskowski's current work on artefacts from Europe showing a high percentage of nickel with silica and slag formations characteristic of bloomery iron. Perhaps space could have been given to the technological development of tools used by different craftsmen such as the need to develop scorpers and engraving tools of steel for the decoration of bronze Boetian fibulae (no discussed by Snodgrass who also omits any reference to metallurgical examination of the iron tools found by Petrie at Egyptian Thebes by A. R. Williams and the writer in *Journal of Archaeological Science*, 3, 1976 283–305). But the important find from Baba Jan of the set of tools probably belonging to a wood-working craftsman (not yet metallurgically examined) is noted by Pigott as well as the need of Urar-

tian stonemasons for hardened cutting tools for hewing irrigation channels through rock and for the production of the well dressed stone blocks used in the temples, tombs and palaces. Pleiner also illustrates and discusses the iron tools used by blacksmiths in the Late Hallstatt and Scythian periods in Europe and includes useful tables to show the applied technology used for different classes of tool.

Finally, the footnotes at the end of each chapter combined with an excellent index provide the student with a splendid picture of the studies and articles written on every aspect of this complex subject from the end of the last century till 1980 and it is hard to find any lacunae. The 400 weapons in the Alalakh texts referred to by Muhly and Waldbaum were not undoubtedly made of iron as AN.BAR was a restoration in a broken text, although, as the list of ŠUKUR weapons comprises three of bronze, five of copper and 400 of an unknown material, iron is a likely candidate. A. H. Lawrence's reference to iron obtained by smelting the hard concretions of laterite for use by Portuguese blacksmiths in the Gold Coast (*Trade Castles and Forts of West Africa*, 31) could be added to the reference to Cuban laterite and references to iron smelting in Ghana. Košak's *Inventory of Mannini* (CTH504), the first published result of his current work on metal in the Hittite texts, published in Ljubljana (1978), and his translation of AN.BAR.GE₆ as 'black iron', is relevant to discussions about the different qualities of iron. A suggestion for its possible use was recently made by the writer in *Anatolian Studies* (1981). The effect of major political events on the spread of technological innovation perhaps could have been treated in more detail, and the omission of a chapter on India is surprising. It would be useful to compare the detailed information about smelting techniques, forging and the organisation of iron production as practised by the Agaria in the Central Provinces (as recorded by Verier Elwin in 1942) with the ethnographical evidence from West Africa given or referred to by Van der Merwe. This is especially relevant as we know hardly anything about the actual organisation of iron production in Europe or Asia in the second or first millennia BC either in its early experimental stage or at the time of large-scale production of implements. We know, in fact, far more about the organisation of the Assyrian army than about the methods of mining, smelting, transport and workshop organisation of the vast supplies of iron used by the blacksmiths for the production of weapons for the soldiers. This contrasts sharply with the detailed picture of the activities of bronze and other metal smiths in the temple workshops of Ur at the beginning of the second millennium BC provided by the economic texts. It is also salutary to remember that one single government decree by Queen Elizabeth in 1596 was responsible for the complete cessation (until 1650) of iron production in the bloomeries of the Furness district of Cumbria in order that the woods should be preserved. The part-time smelters, the shepherds, like Lechtmann's Andean herders, then returned to their flocks. Yet in the Near East we are still woefully ignorant of the effect of large-scale invasion, the transport of craftsmen as practised by the Assyrians, or even of the movement of highly skilled smiths along well-travelled trade routes, although many have speculated on these matters. But *The Coming of the Age of Iron* is happily free of flights of fancy and the basic evidence is admirably presented for all to 'read, mark, learn and inwardly digest' with gratitude.

K. R. MAXWELL-HYSLOP

BIRLA ARCHAEOLOGICAL AND CULTURAL RESEARCH INSTITUTE. *Research Bulletins: Contributions to Archaeological Chemistry*. Hyderabad, Andhra Pradesh, India, 1979.

The Birla Archaeological and Cultural Research Institute was established in Hyderabad in 1969, and it extended its scope by the addition of a Division of Scientific Studies in 1979; it is the research of this Division which is reported in the *Research Bulletins*. So far three issues have been received, at approximately yearly intervals. The contents have so far comprised papers mainly in the field of materials conservation, especially of stone, but two articles have appeared reporting analytical research, one on Śātavāhana potin coins and one on Indian temple bells, and there is also a report on the dating of bones by their fluorine content.

The standard of this publication is high in comparison with many other Indian publications; the illustrations are for the most part clear and legible, though the photographs are variable in quality, and rather few. The unfortunate errors of typesetting which are all too abundant in the majority of Indian publications are pleasantly sparse. The practice of prefacing articles with an abstract is laudable, but the presence of a summary as well is gratuitous; a single more informative abstract is of more use to the casual reader.

BOOK REVIEWS

Progress reports from research institutions are always welcome as a means of disseminating work which might otherwise find no suitable outlet; there are no journals in India which cater specifically for archaeological chemistry, and in the past analytical work has appeared in diverse publications, either archaeological (e.g. *Journal of the Asiatic Society of Bengal*) or scientific (e.g. *Current Science*), and conservation reports have appeared in journals abroad. It is to be hoped that other institutions in India may be moved to follow the example of the Birla Institute; and that the *Research Bulletins* continue to be produced at regular intervals.

P. J. TURNER

WEST, R. G. *The pre-glacial Pleistocene of the Norfolk and Suffolk coasts*. With contributions by P. E. P. Norton, B. W. Sparks and D. G. Wilson. Cambridge, Cambridge University Press, 1980. xi + 203 pp., 54 figs., 36 pls., 49 tables. £40.00.

Exposures along the East Anglian coast of pre-glacial Pleistocene deposits were first studied by Clement Reid, who recognised and named the Cromer Forest Bed Series, over 100 years ago. This sequence has subsequently been recognised to be more complicated than he realised, only the Lower Freshwater Bed represents the Cromer Forest Bed while the Upper Freshwater Bed represents the Cromerian interglacial *sensu lato*. All this coast is subject to constant erosion and most of Reid's type sections are now lost. Continuous re-examination of the coast line has, therefore, been essential whenever new sections became available and the exposures now visible at West Runton and Beeston are here proposed as the Middle Pleistocene stratotypes.

This excellent monograph represents 20 years work by Professor West who has refined the stratigraphic sequence and added considerable further details. He distinguishes two main Middle Pleistocene sedimentary units: the lower Norwich Crag Formation comprising the pre-Pastonian *a* cool stage and the upper Cromer Forest Bed Formation comprising the pre-Pastonian *b* cool, Pastonian temperate, Beestonian cool, Cromerian temperate and Anglian glacial stages. Since this manuscript was completed he has identified a further temperate stage, the Bramertonian, prior to the pre-Pastonian within the Norwich Crag Formation in the late Lower Pleistocene (West 1980: 580-1).

First the analytical techniques employed are described. All lithostratigraphic sections were mapped at a scale of 1:25, located on 1:10,000 or 1:2,500 maps and tied to Ordnance Survey triangulation points. This coast has been subjected to considerable post-depositional isostatic and glacio-tectonic warping and such precision is necessary to correlate widely dispersed deposits. Then the evidence for the pre-Pastonian *a* cool stage is discussed and the lithological, geomorphological and sedimentological history for each site locality is discussed before the biostratigraphic evidence upon which all the reconstructions of past vegetational history are based is described. There is, therefore, no need for an index. Not unnaturally palynological evidence takes pride of place but attention is also given to mollusca and plant macrofossils wherever possible. The clear exposition of the methodology of biological analysis and the problems of pollen assemblage taphonomy, should be read by all students of environmental archaeology.

Eustatic fluctuations during the Middle Pleistocene reflected in lithological facies change and biotic composition are discussed in the light of probable relative isostatic movement and local tectonic deformation. In summary, the evidence suggests that the coastline was relatively stable at this time since the only evidence for sea levels slightly higher than the present tidal range is during the Beestonian and late Cromerian while a marked transgression (+ 23 m OD) first occurs in the Hoxnian interglacial in the early Upper Pleistocene.

It is generally recognised in British Quarternary studies that the preserved lithostratigraphic sequence is discontinuous compared with the more complete record preserved in the Netherlands (West 1977: 266-70). However, the problem is to identify the chronostratigraphic location of the discontinuities. While Professor West has not solved this problem his detailed study clarifies several pertinent points. For example, the terminology of the late Middle Pleistocene merits complete revision. The Cromerian is clearly a multiple event but the stage name is applied to different events on either side of the North Sea.

To have the many figures and tables reproduced in a separate booklet is very useful since it is impossible to comprehend the text without constant reference to them. However, the pocket housing

is a sufficiently nice fit that the booklet will quickly deteriorate with frequent use. This monograph is superbly produced and illustrated and will undoubtedly become a classic work on a classic area. However, while the high quality binding is a fitting tribute to Professor West's lifework, regrettably the resultant prohibitive price will put this book beyond the reach of all but specialist libraries. This is unfortunate since it repays prolonged study and should be required reading for all serious Quaternary students.

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ESMÉE WEBB

Hollstein, E. *Mitteuropäische Eichenchronologie* (Trierer Grabungen und Forschungen XI). Mainz, Philipp von Zabern, 1980. xi + 273 pp., 67 figs. DM135.00.

This volume reports a major breakthrough in European archaeology: the completion of a continuous tree-ring time scale for central Europe back as far as the 7th century BC. Until recently the importance of tree-ring sequences, not only for calibrating other, imprecise chronologies but for direct chronology in its own right has been sadly neglected in the Old World. Hollstein's achievement, effectively through individual enterprise, persistence and skill, should now encourage others to extend this chronology backwards in time and outwards from his main research area around the Rhine Valley.

This monograph includes an account of Hollstein's methodology for dealing with tree sections, for example where sap-wood does not survive, and full graphs of the sequence of sections used to build up the complete time-scale. However, most of the text describes the context and significance of an impressive range of archaeological samples, together with their suggested dendro dates.

From this vast storehouse of new information, results from just two key prehistoric sites may be singled out to illustrate the importance of Hollstein's work. At Villingen in the Black Forest, wooden structures and artefacts reveal the history of a huge tumulus burial complex. A first burial chamber (grave 1) was constructed in 551 BC with secondaries 72/4 in 536 and 6 in 525. A wooden spade used by robbers of the central chamber is dated to 504 BC. These samples had been reported in some detail in 1972 (*Germania*, 50: 69 ff.) and are indeed merely summarised in this volume. However, in 1972 the then published dates were calculated from a floating sequence dated absolutely merely by a supposed historical link – timbers from a bridge at Cologne attributed to a Constantinian construction of AD 310. In the interim, Hollstein managed to link the earlier floating sequence to the series of timbers that leads up to the present day, and the historical equation was found to be 26 years out, with the relevant timbers dating in fact from AD 336, not 310. This means correcting by 26 years all pre-7th century AD dates released by Hollstein before 1976. For Villingen, this revision is not entirely welcome to many archaeologists: grave 1, generally attributed to very early in the Hallstatt D phase, would be located by standard, although not very well founded cross-dating arguments, to the beginning rather than the middle of the 6th century BC. The earlier Hollstein date of 577 BC was closer to this evidently incorrect archaeological supposition.

A happier outcome may be reported for one of the exciting dates provided by Hollstein for the sites at and around La Tène. After detailed historical and archaeological arguments, de Navarro in 1974 (*The finds from the site of La Tène*, Vol. I) cited the initial Hollstein date of 256 BC for a Middle La Tène wooden shield and concluded 'If, as is likelier than not, the above-mentioned shield had a band shaped iron boss, my dating for such shield bosses . . . and with it probably the upper limit of Middle La Tène in north Switzerland, would have to be raised by about a generation' (*op. cit.*, p. 354). The revised Hollstein date, as close to a generation later as could possibly be discerned, provides impressive testimony to the accuracy which traditional methods of dating may achieve when skilfully applied.

With the present dearth of such archaeological skill, it is fortunate that Hollstein and his successors will be able to provide direct absolute dates for the transition from prehistory to history in Germany and beyond, perhaps before long even as far as the British Isles.

F. R. HODSON

BOOK REVIEWS

PIGGOTT, Stuart, *Antiquity depicted: aspects of archaeological illustration*. London, Thames and Hudson, 1978. 64 pp., 43 pls. £3.50.

Antiquity depicted is the published version of a lecture with the consequent restricted scope that this implies; true to its subtitle, it represents merely aspects of archaeological illustration and not a complete history. Within these limitations Professor Piggott uses specific illustrations from six centuries to demonstrate phases in the progression towards a visual language for antiquarian subjects and places his examples within the changing arena of scientific and philosophical thinking from which the discipline of archaeology itself was to emerge. He looks firstly at the portrayal of standing monuments as curious and incidental parts of landscape when artistic licence rather than scientific observation was uppermost and demonstrates how gradually 'the first hand empirical study of phenomena' became the governing force. He shows a similar development in the illustration of artefactual material from portrayal as art objects to precise recording for the needs of taxonomy. More emphasis might have been laid on the growing importance of illustration as a tool for cataloguing and for recording otherwise ephemeral data from excavation and survey and less on the early depiction, however diverting, of archaeological material for its strange and outlandish qualities. Professor Piggott does not make a clear distinction between representations as art and those as scientifically observed data, perhaps because his sympathies lie with those areas where the boundaries are most blurred.

He finishes somewhat abruptly by dismissing completely all the work since the 1930s as so much 'misused Letraset'. Since this must represent the bulk of all known archaeological illustration it is bound to contain material that is less than praiseworthy but even a cursory glance through the books and periodicals of only the last 10 years would show as well as indifferent use of the available visual language, numerous examples of scientific data presented in a pleasing manner. Stuart Piggott's own sections and isometric drawings, modestly discounted with the rest of the last 50 years, alone might prove that the development he charts did not end with Mortimer Wheeler.

One point needs positive correction; Ill. 31 (p. 46) is not in fact 'a woeful travesty' of the Bridgeness slab (Ill. 30 above) but a precise rendition of the Summerston slab from the opposite end of the Antonine Wall.

A. N. SHEPHERD

DANIEL, G. (ed.) *The illustrated encyclopaedia of archaeology*. London, Macmillan, 1978. 224 pp., illus. £8.95.

SHERATT, A. (ed.) *The Cambridge encyclopaedia of archaeology*. Cambridge, Cambridge University Press, 1980. 495 pp., illus. £18.50.

The publication within the space of three years of two encyclopaedias of archaeology testifies to the vital importance of informing the public of the significance and achievements of archaeology in illuminating of the story of mankind. In their separate and largely complementary quests for clear and stimulating communication, both works are notably successful; but the end-products could hardly have presented two more divergent faces of the discipline had they been written on separate planets.

In his introduction to the *Illustrated encyclopaedia* (IEA), Professor Daniel laments the principal difficulty of compiling such surveys – namely the existence of 'so many archaeologies and so many differing archaeologists' (pl. 6). This fact is nowhere more clearly seen than in the lists of contributors to each book; only one scholar wrote for both books and one further specialist sat on both editorial boards. This contrast is mirrored in almost every aspect of the two volumes, but most strikingly in the financial framework and the underlying archaeological philosophies.

Any serious attempt at comparison of these encyclopaedias must recognise the greater potential resulting from the CEA's more ample financial base. The contrast in styles is, as it were, between a Masarati and a Mini Metro, with the inevitable differences in visual appeal, acceleration and also safety margins. The only colour illustrations in the IEA occur on the cover, whilst colour figures constantly enliven and enrich the CEA. Indeed, the colour schemes of the eight chronological charts and 36 maps at the end of the CEA communicate succinctly the contemporary trends in world cultural development.

But the presence or absence of attractive pictures pales into insignificance compared to the philosophical gulf between the two works. In a phrase almost too apt to be coincidental, the stated aim of the IEA is 'to reflect the great diversity of the field rather than to set forth a single and controversial view' (p. 6). By contrast, the CEA presents just such a unified, more controversial synthesis of world culture process. The differing formats of the two books are essentially logical outgrowths of this contrasting approach.

The IEA attempts to avoid conclusions biased by any single theoretical outlook by adopting the traditional format of entries on topics arranged alphabetically. These entries comprise 95% of the book, whose only consolidated, explanatory text is a two-page introduction by Professor Daniel. In this section, there is an unhesitating distinction between 'fact' and 'theory' rooted in the artefact-based approach of Daniel's generation. The claim that 'every artefact is the vital concern of the archaeologist, from the earliest pebble-tools . . . to yesterday's garbage' is merely an extension to the present of a minimalist archaeological standpoint dubbed the 'sceptical tradition' by Klejn, in which the impossibility of social and economic reconstructions for prehistory leads to almost total devotion to the primary data. Indeed, the tone of the book is perfectly captured by the cover design, in which three prestige artefacts are inset against an evocative sunset with heavy romantic overtones.

Yet the pure a-theoretical position is in itself an impossibility, and on the same page we discover the true theoretical basis of the IEA: 'the ultimate aim of (the archaeologist's) craft and his use of the physical and biological sciences is to write human history', even in prehistory (i.e. 'pre-written history'). This historical basis of the subject provides a reference point for the entries relating to regional topics or themes such as urbanism or the Neolithic.

The CEA reflects Colin Renfrew's belief (p. 77) that 'it is the nature of the problem and the clarity of the theoretical response that principally governs the quality of the resultant work'. The book is composed of 64 chapters summarising those themes considered significant in modern archaeology. The work is divided into three parts. Part One, comprising seven chapters, reviews the historical and theoretical developments of modern archaeology; in Part Two, major themes such as Man the Hunter, the Postglacial Revolution, Old World Empires and New World Developments are explored on a chronological or regional basis in 54 chapters; and in Part Three the chapters on dating techniques and cultural distribution buttress the middle section to a firm culture-historical framework.

A magisterial foreword by Grahame Clark highlights the CEA's view of the subject's contribution to the biological and social sciences, namely 'a dramatic extension of the range of human awareness' (p. 8). This extension stems not only from the discovery of new primary data but also, as Andrew Sherratt suggests, from 'controlled comparison, the recognition of regularities in the development of human societies in diverse circumstances: (p; 9). By enquiring into the broadest problems of long-term human change, the CEA has adopted the antithesis of Piggott and Daniel's sceptical tradition. Almost by definition, this quest is more stimulating than agnostic archaeology, yet this presents its own inherent methodological drawbacks.

The two principal criticisms of the CEA approach concern the balance between data and interpretation and the compression of complex issues. In the first instance, it is perhaps inevitable that problem-orientated studies raise more questions than answers. Yet the tendency to over-interpret the data to achieve a theoretically satisfying answer is often hard to resist and can lead to the conflation of fact with fancy. Whilst professional archaeologists may find it easier to separate speculation from sound inference, undergraduates and the general public may find such an operation difficult, especially if similar, self-reinforcing types of interpretations are plausibly presented in successive chapters. The related question of selectivity hinges upon whether a specialist can say anything useful about topics such as Mesoamerican prehistory or the Hellenistic world in a handful of pages. For example, one page of text on the European Bronze Age seems ultra-minimal. Again it is the generalist who is at the mercy of the specialist's choice of which side of a debate to favour, which theory to construe as generally accepted and indeed which cultural developments to include in the first place.

Nevertheless, the feeling of the heady intellectual ferment in modern archaeology permeates the text of the CEA. Nowhere is this excitement caught so aptly as in the chapter on earliest man, with hominid remains at 5.5 million years bp, the extraordinary Laetoli footprints and 'Lucy' herself. A more sober but most scholarly assessment of the development of the earliest culture of modern man is one of the highlights of the book, in which sound discussion of key points (e.g. the Mousterian controversy) is combined with imaginative treatment of the biological basis for further development. The chapters discussing the origins of food production lay stress on the variability of

response to Neothermal conditions. The early and remarkable contributions of Japan are amply illustrated – the use of edge-ground axes by 28,000 bc, micro-blades by 12,000 bc (pre-Jomon), pottery by 9,000 bc and sporadic domestic plants by 5,500 bc. However, amongst such a mass of valuable material, factual errors occasionally creep in. In the section on Near Eastern agricultural origins, for instance, it is misleading to conclude that a stable agricultural base resulted in larger, permanent villages (p. 105); in fact, quite the reverse process was indicated by P. E. L. Smith a decade ago.

In the many chapters on the emergence of complex, urban societies, there is a recurrent neo-diffusionist theme. In the editor's final chapter, a modified Wallersteinian model of core-periphery development picks up the threads spun in several earlier chapters, on the Near East (where Sumer and Elam are thought to have 'exported' their urban concept through trade), on the Aegean (diffusion from NW Anatolia to the Aegean in the Early Bronze Age) and Mesoamerica (the successive influence of the Olmec and the city of Teotihuacan on surrounding territories). Yet here the structural parallels stop and due emphasis is given to the formative influence on urban origins of local modes of socio-economic organisation in relation to environmental differences.

Profound insight into the pace of change forms one of the most lasting impressions of the CEA. In the Introduction, Sherratt writes that 'Archaeology demands a logarithmic imagination successively to encompass the rates of change in world and human development'. This logarithmic framework illuminates many chronological diagrams, but nowhere more imaginatively than in the representation of the Linearbandkeramik culture of Neolithic Europe – from continental distribution to house-plan in five stages (Fig. 20.5, p. 149). Indeed, the ability of the CEA to recognise the broad issues of cultural development is at once the book's greatest strength and weakness; many innovations deserve more attention than the global perspective is able to afford, yet the total picture is presented with clarity and skill.

The IEA starts at the opposite end of the spectrum of specificity. Its most impressive aspect is the breadth of coverage, with sections on the Ertebølle culture and Aquileia as detailed as on electricity and the oil industry. This artefactual modernism, rarely consonant with an a-theoretical perspective, reaches a climax when, in contrast to eight lines on the Australopithecines, 21 lines are devoted to the automobile industry. Happily, balance and good sense are rapidly restored with 33 lines on Avebury, although controversialists will lament the absence of the Dorians.

Some of the most useful general accounts provide introductory regional coverage. Some sections, as on Anatolia, are well served by maps and photographs, and the entry on Ireland is particularly clear. An otherwise excellent East Europe entry is, however, marred by the ill-informed and tendentious attack on dialectical materialism in the last paragraph; and two columns for the archaeology of Italy seems a trifle under-zealous.

True to its philosophical basis, the IEA parades a darker streak when discussing the theory of other archaeologists. The entry on anthropology and archaeology centres on the out-moded discussion of ethnographic parallels without mention of the emerging field of ethno-archaeology. Predictably, perhaps, the most stringent cries are directed at 'New Archaeology' and its high priest, Lewis Binford. Half the ideas credited to Binford in this section do in fact originate from David Clarke, whilst other strengths of processualism are listed as 'an evolutionary view of culture', 'speculative reconstruction of social and religious life' and 'the calculation of absolute dates' (p. 154)! It is perhaps unfortunate that a neutral observer could not have read such sections before press.

Theory aside the greatest value of the IEA is its culturally-based sections, in which valuable material is arranged by thorough cross-referencing which is easy to use. There are remarkably few printing errors considering the frequency of proper names (e.g. Androvonovo for Andronovo, p. 19). The only additional feature of benefit would be an annotated bibliography listing general works and regional studies; yet the absence of references in the text is certainly an aid to clarity.

In conclusion, both books mark a welcome contribution to the general archaeological literature. Their evident complementarity arises out of the philosophical chasm between the two works. The IEA provides sound summaries of the cultures and sites alluded to all too briefly in the CEA, whilst the CEA makes up for the theoretical caution of its sibling by global processualist synthesis. Individual preference for either volume will depend entirely on that very diversity of viewpoints which is one of the hallmarks of human behaviour.

JOHN CHAPMAN

BOOK REVIEWS

NEILL, W. T. *Archaeology and a science of man*. New York, Columbia University Press, 1978. ix + 321 pp., 52 pls. \$25.00.

The author teaches biogeography at a community college in Florida and is a herpetologist. That he is not a trained archaeologist is clear from the inaccuracies and misplaced emphasis in some chapters. His main thesis, which was hardly original even in 1978, is that archaeology is the *science of man* and should be an interdisciplinary study utilising linguistic, ethnographic, place name, physical anthropological, socio-economic and environmental specialist information to facilitate problem solving. He equates archaeology with material culture which he argues is a chronologically continuous social product and, therefore, the only difference between history and prehistory should lie in analytical methodology. However, many archaeologists would consider that difference fundamental! His argument seems a little naive especially since he misuses terms like *human ecology* and he falls into the Socratic fallacy of assuming that knowledge entails improvement. He argues that much past poor archaeological work was due to ignorance of the *real* aims of archaeology; possibly, but on-site constraints and shifts in archaeological emphasis should also be considered.

In Chapter 4 his discussion of basic physical anthropological analyses (sex, age, hair/skin types, pathology, histology, parasitology, etc.) is very simplistic. Some of the examples he cites of the use of bone pathologies to trace human migratory movements, dietary deficiencies and cultural behaviour, for example tooth expulsion, cranial deformation or phalange excision, are more useful. However, his description of the earliest Amerind populations omits all discussion of problematic material, for example, the presence of Neandertals in South America is improbable, and renders bland a highly controversial topic.

It is in Chapter 5 that the worst errors occur, due largely to Neill's ignorance of work on African material published since 1970. He makes no reference to the work of the Leakeys, Isaac or Desmond Clark, but accepts unquestioningly Dart's papers on the tool-making abilities of *A. africanus* based on the dubious material from Makapansgat. He confuses the important difference between *stone* and *flint* and persistently refers to the presence of *flint* tools from Olduvai Beds I and II where almost every other stone type was used as raw material but not flint. In a simplistic discussion of hominid evolution he seems unaware of the presence of *Ramapithecus* in Kenya and he accepts other taxonomic labels at face value with little understanding of the problems of Plio-Pleistocene hominid phylogeny. Even supposing the manuscript was completed in 1975 it is strange to find no reference to marine stable isotope research and the old quadripartite glacial model put forward unopposed. In his discussion of Neandertal man two points merit comment: he labels all signs of religious belief, for example burial and (possibly) art, as 'fiction' and 'myth', rather an unhelpful and anachronistic attitude; was cerebral excision really *common*?

Chapters 7–9 deal with environmental archaeology, its past uses and future potential. Reading them an Old World archaeologist has a sense of *déjà vu* which is confirmed by a glance at the bibliography. Most of the volumes cited were published prior to 1970, a few 1975 publications are listed. Neill's unfamiliarity with the more recent literature robs his argument of much of its force since many of the approaches which he advocates had become commonplace by the time of publication. There are some startling statements: he offers the hypothesis that alkaline soil conditions are possibly conducive to bone preservation! His discussion of dating techniques is over-simplistic, and he uncritically accepts the dendrochronological recalibration of radiocarbon.

It is difficult to decide at what audience this book is really aimed. It was probably intended for the American college market. However, it is too specialised for the general humanities student while being far too generalised for the archaeology student, although they may find instructive the more detailed case studies of four problems in American archaeology in the last chapter, possibly the best part of the book.

ESMÉE WEBB

HUDSON, Kenneth. *Museums for the 1980s: a survey of world trends*. Paris, UNESCO; London, Macmillan, 1977. 198 pp., illus. £20.00.

This book is about the democratisation of museums – or, rather, the arguments for this and an account of attempts to achieve it. It is aimed at museum professionals everywhere and especially in the developing countries.

BOOK REVIEWS

Kenneth Hudson is undoubtedly the man to present the subject and, what is more, he believes in it. As UNESCO museums consultant he has had unrivalled opportunity to study museums round the world and in particular the evolving museum movement in developing countries where museums are essentially seen as part of community activity and self-awareness. It is instructive that Japan now has the second largest number of museums of any country in the world (being exceeded only by the USA).

Theory and argument are deployed in the long Introduction (16 three-column pages). Examples of practice and discussion are organised under the headings: The Museum's Resources; Conservation; Museum Buildings; The Museum and its Visitors (with three appendices); Selection and Training of Personnel; Museum Management. There is an index and a delightfully quirky glossary. It is natural that Chapter 4, The Museum and its Visitors, is the lengthiest section turning up some 'shocking' statistics, such as that most of Britain's national museums had (at least till 1976) never carried out a visitor survey. Much of the material in this chapter comes from widely distributed questionnaires.

Mr Hudson almost persuades us that this desirable objective – democratisation of museums – is unobtainable. At the same time, the attempt on this unobtainable is the only guarantee of a museum's survival in the next two decades. Classic curatorship is primarily at risk from the new thinking where the successful director is likely to be an entrepreneurial person rather than a scholarly person. The disconcerting effect overall of the transformation envisaged is that museums will cease to be recognisable in the form they have come to be known since their invention four or five generations ago, and, both word and concept will be swallowed up under such labels as 'Cultural Centre' or 'Culture Workshop'.

Despite the author's iconoclasm there is much good sense in these pages and many entertaining and informative passages. The idea that the traditional gallery wardens should double as feedback channels on visitor reaction seems well worth following up. Mr Hudson is particularly eloquent on museum buildings, contrasting the Victorian 'temples of scholarship and self-improvement' housed in superannuated palaces or a new building designed to look like a palace where the visitor must toil upwards to begin the tour 'to induce a suitably elevated state of mind' with the modern idea of a democratic building. This is a building which says to people – 'This is your kind of building, designed, constructed and fitted out with you in mind' and so 'a democratic building first and foremost' of which the airport is the type.

Museums for the 1980s brings together and moulds a mass of information that it would be hard to find elsewhere. Where would one turn to discover between the same covers descriptions of the salt seep problem at the Prince of Wales Museum, Bombay; traditional African games at the Sukuma Museum, Tanzania; the Yeh S'ing memorial village in Kiangsu province, China; the Am Berd site museum in Soviet Armenia, and the Snake Park, Nairobi?

ANGELA CROOME

BOOKS RECEIVED

The following books have been received. The fact that they are listed here does not preclude their review in a later issue.

BAYNES-COPE, A. D. *Caring for books and documents*. London, British Museum, 1981. 32 pp., illus. £2.50.

BLÁZQUEZ, José María. *Castulo II* (Excavaciones arqueológicas en España 105). Madrid, Ministerio de Cultura, 1979. 450 pp., 186 figs., 59 pls., 20 plans. Ptas. 3,000.

BLÁZQUEZ MARTÍNEZ, José María and VALIENTE MALLA, Jesús. *Castulo III* (Excavaciones arqueológicas en España 117). Madrid, Ministerio de Cultura, 1981. 300 pp., 155 figs., 28 pls. Price not stated.

BLÁZQUEZ, José María. *Mosaicos romanos de Cordoba, Jaen y Malaga* (Corpus de Mosaicos de España 3). Madrid, Instituto Español de Arqueología 'Rodrigo Caro' del Consejo Superior de Investigaciones Científicas, 1981. 133 pp., 32 figs., 62 pls. Price not stated.

BLÁZQUEZ, José María. Los mosaicos romanos de Torre de Palma (Monforte, Portugal). *Archivo Español de Arqueología*, 53 (1980), pp. 125–150, 19 figs. Price not stated.

BOOK REVIEWS

- BONEFANTE, Larissa. *Out of Etruria: Etruscan influence North and South* (BAR Int. Ser. 103). Oxford, British Archaeological Reports, 1981. ii + 173 pp., 122 pls., 10 maps. £10.00.
- BOWEN, E. G. *Britain and the western seaways*. London, Thames and Hudson, 1981. 196 pp., 105 illus. Paperback edition, £4.50.
- BRENT, Peter. *Charles Darwin: 'a man of enlarged curiosity'*. London, Heinemann, 1981. 563 pp., 28 pls. £12.50.
- CLARE, T. *Archaeological sites of the Lake District*. Ashbourne, Moorland Publishing Co., 1981. 159 pp., illus. £7.50; Paperback £4.95.
- DANIEL, Glyn. *A short history of archaeology*. London, Thames and Hudson, 1981. 232 pp., 146 illus. £9.50.
- GROOM, Nigel. *Frankincense and myrrh: a study of the Arabian incense trade*. London, Longman, 1981. xvi + 285 pp., illus. £14.95.
- HAMMOND, N. G. L. (ed.). *Atlas of the Greek and Roman world in antiquity*. Park Ridge, NJ, Noyes Press, 1981. viii + 56 pp., 56 maps. \$48.00.
- HARDEN, Donald B. *Catalogue of Greek and Roman glass in the British Museum*. Vol. 1. *Core and rod-formed vessels and pendants and Mycenaean cast objects*. London, published for the Trustees of the British Museum by British Museum Publications, 1981. 187 pp., 19 figs., 29 pls., 3 tables. £55.00.
- HAYES, J. W. *A supplement to 'Late Roman pottery'*. London, British School at Rome, 1980. pp. 479–551, illus. £4.50.
- HEGGIE, D. C. *Megalithic science: ancient mathematics and astronomy in Northwest Europe*. London, Thames and Hudson, 1981. 256 pp., 111 illus. £12.00.
- HELMIS, S. W. *Jawa: lost city of the Black Desert*. London, Methuen, 1981. xviii + 270 pp., illus. £14.95.
- HODDER, I. *Symbols in action: ethnoarchaeological studies of material culture* (New Studies in Archaeology). Cambridge, Cambridge University Press, 1982. x + 244 pp., 87 figs. £19.50.
- HOOD, Sinclair, and SMYTH, David. *Archaeological survey of the Knossos area* (British School at Athens supplementary vol. 14). London, British School at Athens; Thames and Hudson, 1981. x + 69 pp., illus., map. £19.00.
- HOOD, Sinclair, and TAYLOR, William. *The Bronze Age palace at Knossos: plans and sections* (British School at Athens supplementary vol. 13). London, British School at Athens; Thames and Hudson, 1981. xiv + 34 pp., illus., plans. £14.00.
- JONES, G. D. and KAUTZ, R. R. *The transition to statehood in the New World* (New Directions in Archaeology). Cambridge, Cambridge University Press, 1981. xi + 254 pp., 12 figs. £17.50.
- KING, A. and HENIG, M. *The Roman west in the third century: contributions from archaeology and history* (BAR Int. Ser. 109), 2 vols. Oxford, British Archaeological Reports, 1981. 538 pp., illus. £20.00.
- LAUGHLIN, W. and HARPER, A. B. *The first Americans: origins, affinities, and adaptations. Proceedings of a conference held at the European Conference Center of the Wenner-Gren Foundation for Anthropological Research, Burg-Wartenstein, Austria, 21–30 August 1976*. Stuttgart, Gustav Fischer, 1979. xi + 340 pp., illus. DM 52.00.
- LAVALLEYE, J. *Introduction à l'archéologie et à l'histoire de l'art* (Publications d'histoire de l'art et d'archéologie de l'Université Catholique de Louvain 17), 4th edn. Louvain-le-Neuve, Institut Supérieur d'Archéologie et d'Histoire de l'Art, 1979. xxii + 221 pp., 36 figs. Price not stated.
- MARINESCU-BÎLCU, S. *Tîrpești: from prehistory to history in Eastern Romania* (BAR Int. Ser. 107). Oxford, British Archaeological Reports, 1981. 187 pp., 245 figs., 15 pls. £20.00.
- MERCER, Roger (ed.). *Farming practice in British prehistory*. Edinburgh, Edinburgh University Press, 1981, xxvi + 245 pp., illus. £9.50.
- MORRIS, John. *Londinium: London in the Roman Empire*. Revised by Sarah Macready. London, Weidenfeld and Nicolson, 1982. xvi + 384 pp., 16 pls., 14 maps. £15.00.
- NATURAL HISTORY MUSEUM OF LOS ANGELES COUNTY: *Sweat of the sun, tears of the moon: gold and emerald treasures of Colombia*. Los Angeles, Terra Magazine Publications, 1981. 96 pp., 48 pls. \$10.00.
- PARTRIDGE, Clive. *Skeleton Green, a late Iron Age and Romano-British site* (Britannia Monograph Series 2), London, Society for the Promotion of Roman Studies, 1981. 359 pp., 137 figs. £12.50.

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- PIGGOTT, Stuart (ed.). *The agrarian history of England and Wales*. Vol. 1/1. *Prehistory*. Cambridge. Cambridge University Press, 1981. xxi + 451 pp., illus. £27.50.
- RATNAGAR, Shereen. *Encounters: the westerly trade of the Harappa civilization*. Delhi, Oxford University Press, 1981. xxi + 294 pp., illus. No price stated.
- READER, J. *Missing links: the hunt for earliest man*. London, Collins, 1981. 274 pp., illus. £9.95.
- ROE, Derek A. *The Lower and Middle Palaeolithic periods in Britain* (Archaeology of Britain 3). London, Routledge & Kegan Paul, 1981. xvi + 324 pp., illus. £35.00.
- SHERIDAN, Alison and BAILEY, Geoff. (eds.). *Economic archaeology: towards an integration of ecological and social approaches* (BAR Int. Ser. 96). Oxford, British Archaeological Reports, 1981. 303 pp., illus. £11.00.
- SIMMONS, I. and TOOLEY, M. (eds.). *The environment in British prehistory*. London, Duckworth, 1981. x + 334 pp., illus. £24.00; paperback £7.95.
- SIMPSON, Richard Hope. *Mycenaean Greece*. Park Ridge, NJ, Noyes Press, 1982. xx + 246 pp., 15 figs., 32 pls. \$45.00.
- STADELMANN, Jutta. *Der Runde Berg bei Urach IV: Funde der vorgeschichtlichen Perioden aus den Plangrabungen 1967-74* (Schriften/Heidelberger Akademie der Wissenschaften, Kommission für Alamannische Altertumskunde 7). Sigmaringen, Jan Thorbecke, 1981. 109 pp., 82 pls. DM 120.00.
- TRANOY, A. *La Galice romaine: recherches sur le nord-ouest de la péninsule ibérique dans l'antiquité* (Publications du Centre Pierre Paris (E.R.A. 522)) (Collection de la Maison des Pays Ibériques (G.I.S. 15)). Paris, Diffusion de Boccard, 1981. 602 pp., 13 figs., 16 pls., 36 maps, 18 tables Fr. 480.000.
- WILSON, David M. (ed.). *The archaeology of Anglo-Saxon England*. Cambridge, Cambridge University Press, 1981. xvi + 532 pp., illus. Paperback edition £9.95.
- ZVELEBIL, M. *From forager to farmer in the Boreal zone: reconstructing economic patterns through catchment analysis in prehistoric Finland* (BAR Int. Ser. 115), 2 vols. Oxford, British Archaeological Reports, 1981. 472 pp., figs., maps. £20.00.

Reports*

M.A. and M.Sc. Reports for 1980–81

Glass wastes on Anglo-Scandinavian crucibles from Flaxengate, Lincoln
 Metallographic analysis of metal objects from Palestine
 Pre-urbanism and economy in the Ancient Near East
 Funerary epitaphs and iconography: an analysis of the Protestant Cemetery, Rome
 Technological and functional criteria for the analysis of New York State Late Woodland points
 A study of pottery from Cowdery's Down, Basing, and Viabes Farm, Basingstoke, Hampshire

Kate Foley
 Nayla Ghandour
 Deirdre T. M. Kells
 Sebastian P. Q. Rahtz
 Mara E. Rosenthal
 Anne E. Thompson

B.A. and B.Sc. Reports for 1980–81

Chemical cleaning of copper alloy artefacts – methods and applications
 Architecture of the Decapolis
 Roman theatrical masks and their representations including an investigation of their possible continuity
 A comparison of the settlement patterns of both the Romans and Anglo-Saxons in the areas surrounding Ilchester and Dorchester
 A study of traditional technology in Greece and Cyprus as a demonstration of the value of ethnography to archaeology
 The mineralogy of bronze corrosion
 Aspects of female dress during the Pagan Saxon period in the south of England
 Egyptian gold foil technology and conservation
 A study of the history of the protection of ancient monuments by parliamentary legislation
 Silbury Hill considered as a building project
 Building 20; the excavation of pre- to post-Inca levels at Huillca Raccay, Cusichaca, Peru (season 1979/8)
 A study of Roman catapult artillery and its ammunition from the archaeological remains
 Problems of interpreting trauma
 The Roman villa in Africa Proconsularis

Carola M. Bohm
 Julian M. C. Bowsher
 Sylvia M. Carter
 Belinda A. Chant
 Linda M. Cheetham
 Barbara H. Christen
 Aileen A. Connor
 B. Karen Coote
 Justin M. Day
 Barabara M. Douglas
 Peter C. J. Durnford
 Nicholas J. Elsden
 Robert M. Entwistle
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* Reports are housed in the Institute of Archaeology Library.

REPORTS

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|---|----------------------|
| Development of church furniture from IV to VIII centuries | Susan J. Gill |
| Attribute analysis as a method of studying variability in palaeolithic assemblages | Roger D. Grace |
| Some aspects of bracelets in Britain 43–410 AD | Ann E. Greenhalgh |
| An analysis of the distribution of the coinage of the Emperor Hadrian | Andrew S. Hobley |
| An analysis of some flint flakes and blades from the Nagada region, Egypt | Diana L. Holmes |
| A guide to the conservation of ancient Egyptian wooden funerary models | Richard L. Jaeschke |
| X-ray diffraction of gold-based alloys | Gillian Juleff |
| Museum environment in the tropics | W. A. Lemani |
| Timber buildings from the 1st to 9th centuries AD with special reference to continuity between Roman and Saxon building types | Anne Marshall |
| Hoard coins of S Severus and his family (to AD 217) | A. Ross Millard |
| The economy and monasticism of Later Roman Egypt | Elizabeth J. Nichols |
| A study of Roman coinage across the frontier in South Germany | Daniel J. Merceron |
| The siting and distribution of early Saxon settlement sites and cemeteries in Surrey AD 450–700 | John P. F. Mills |
| La Grotte de Maldidier, Dordogne, France: observations on the Aurignacian and Perigordian stone tools with reference to differential use of siliceous raw materials | Taryn J. P. Nixon |
| The Greek helmet from the mid 5th to the 2nd century BC | John M. Paddock |
| Indus valley seals | Marina Raiss |
| Artificial cranial deformation in SE Asia and Australasia: evidence for the practice of artificial cranial deformation in the Late Pleistocene | Caitlin C. Rhodes |
| The potential of underwater environmental archaeology: Oak Bank crannog, Loch Tay, Scotland – a case study | Judy G. Stokes |
| From ard to plough from pre-Roman to late Anglo-Saxon England | Amanda J. Ward |
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| The late Roman city wall of Touyrs – an architectural study | J. Jason Wood |
| Fire and sword – the techniques of destruction | John K. Wood |

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Notes to Contributors

Papers on any aspect of archaeology may be considered for publication in the *Bulletin*. All contributions and related correspondence should be addressed to the Editor, Professor J. D. Evans, Institute of Archaeology, 31–34 Gordon Square, London WC1H 0PY. The following notes are provided as a guide to intending contributors in the preparation of their material.

1) *Typescripts* should not normally exceed 10,000 words in length. They should be typed on one side only of A4 size paper (approx. 30 cm by 21 cm), using double spacing and leaving wide margins (at least 4 cm on the left). Two copies of the typescript should be submitted, the author retaining a third copy.

2) *Footnotes* should be avoided as far as possible. If any are judged to be absolutely necessary, they should be typed on a separate sheet, not at the foot of the page to which they refer.

3) *Bibliographical references* should follow the so-called Harvard system. The author's last name, date of publication and number of page should be given in brackets in the body of the text, e.g. (Sharma, 1973: 129), or, if the author's name has been cited, simply (1973: 129). Full references should be listed alphabetically according to authors' names at the end of the paper.

e.g. Paper: Sharma, G. R. 1973. Mesolithic lake cultures in the Ganga valley, India. *P.P.S.*, 39: 129–146.

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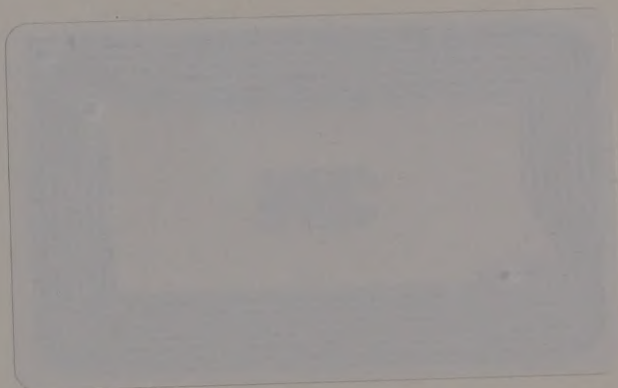
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